AD 246 997

UNITED STATES NAVAL ORDNANCE LABORATORY WHITE OAK SILVER SPRING MARYLAND

NOL AEROBALLISTIC RESEARCH FACILITIES DEDICATION & DECENNIAL

NOLR 1238

25-26 MAY 1959

addresses
at the
dedication
of the
new
aeroballistic
research facilities
and at the
decennial symposium
on
aeroballistics

61-1-3
NOX

62520

NOL

AEROBALLISTIC

RESEARCH

FACILITIES

DEDICATION &

DECENNIAL

UNITED STATES NAVAL ORDNANCE LABORATORY

addresses

at the

dedication

of the new

aeroballistic

research facilities

and at the

decennial symposium

on aeroballistics

25-26 MAY 1959

DEPARTMENT OF THE NAVY
NAVAL ORDNANCE LABORATORY

WHITE OAK SILVER SPRING MARYLAND

ADDRESSES AT THE DEDICATION OF THE NEW NOL AEROBALLISTIC RESEARCH FACILITIES AND AT THE DECENNIAL SYMPOSIUM ON AEROBALLISTICS

25 - 26 May 1959

With a Roster of Persons who Registered
for the Ceremonies

Published October 1960

U. S. NAVAL ORDNANCE LABORATORY
White Oak, Silver Spring, Maryland

NOLR 1238 1 February 1960

This report has been prepared to record the addresses given by our distinguished guest speakers as well as those by the Naval Ordnance Laboratory staff. The complete program for the two-day ceremonies is documented together with the roster of those who registered in attendance. It becomes of historical import as an account of a most significant milestone in the growth of the Naval Ordnance Laboratory, White Oak.

The efforts of Mr. James R. Lightfoot, Assistant Aeroballistics Program Chief for Engineering, who compiled and edited this report, are gratefully acknowledged.

MELL A. PETERSON
Captain, USN
Commander

G. K. HARTMANN
Technical Director

H. H. Kurzweg.
H. H. KURZWEG
Associate Technical Director
for Aeroballistic Research

FOREWORD

After World War II, the Bureau of Ordnance, anticipating an era wherein the striking force of the Navy might include weapons, planes, and ballistic missiles travelling at speeds into the supersonic and hypersonic regions, approved and encouraged the establishment of Aeroballistic Research as an important segment of the Naval Ordnance Laboratory's scientific effort. To implement this conviction, the Bureau of Ordnance concurrently endorsed a program calling for the construction at NOL, White Oak, of several major Aeroballistic Research tools which were to become the nucleus of the Laboratory's current, modern Aeroballistic Research Facilities.

1949: The post war effort mentioned above culminated in the formal dedication on 27 June 1949 of the initial NOL Aeroballistic Research Facility. Since that time the Laboratory has actively participated in advancing the state-of-the-art of aerodynamics, ballistics, and applied mathematics, employing primarily these facilities originally dedicated. History shows that during this decade not only was supersonic flight well established but pioneering into hypersonic flight became a reality.

1959: These new Aeroballistic Research Facilities, dedicated on 25 May 1959, and in the hands of a competent staff with a wealth of experience in research and development in the sciences which constitute the field of aeroballistics, can be expected to play a major role in the development of weapons for the modern Navy as well as in the exploration and understanding of the basic scientific concepts involved.

To further commemorate this significant occasion, on 26 May 1959 a Decennial Symposium was held during which technical papers pertinent to aeroballistics, hyperballistics and allied sciences were presented to an audience comprised mostly of outstanding nationally and internationally known scientists in the field.

This Naval Ordnance Laboratory Report outlines the programs for both days and presents the lectures and talks given by each of the distinguished participants.

CONTENTS

Dedication Ceremony — Monday, 25 May 1959

Chairman:

Dr. H. H. Kurzweg, Associate Technical Director for Aeroballistic Research, NOL

	Page
Facilities Being Dedicated	1
Description and Characteristics of the Three Major New Aeroballistic Research Facilities	
Welcome	9
Captain Mell A. Peterson Commander, NOL	
Presentation of New Aeroballistic Research Facilities . . .	10
Vice Admiral John T. Hayward Deputy Chief of Naval Operations for Development	
Address	13
Rear Admiral W. F. Raborn Director, Special Projects Office, BuOrd	
Response	16
Dr. G. K. Hartmann Technical Director, NOL	
The Aeroballistic Research Facilities at NOL	18
Dr. H. H. Kurzweg	

Page

Contributions of Aeroballistics to Space Explorations 38

Dr. H. L. Dryden
Deputy Administrator, NASA

Evening After-Dinner Talk 46

Dr. Theodore von Karman
Chairman, Advisory Group for Aeronautical
Research and Development, NATO

Decennial Symposium Tuesday, 26 May 1959

Morning Chairman:

Dr. R. J. Seeger
Deputy Assistant Director
Mechanical, Physical and
Engineering Sciences, NSF

Introductory Remarks - Yesterday and Tomorrow 52

Dr. R. J. Seeger

High Temperature Gas Dynamics 57

Dr. J. M. Burgers
Institute for Fluid Mechanics and Applied
Mathematics, University of Maryland

Survey of NOL Aerodynamic Research 87

Dr. R. K. Lobb, Chief
Aerodynamics Department, NOL

Survey of NOL Hyperballistic Research 106

Dr. Z. I. Slawsky, Chief
Ballistics Department, NOL

Page

Recent Advances in the Mechanics of Highly Rarefied Gases . 124

Dr. G. N. Patterson, Director
Institute of Aerophysics, University of Toronto

Survey of NOL Applied Mathematic Research 144

Dr. R. C. Roberts, Chief
Mathematics Department, NOL

Effects of Mass Transfer on Boundary
Layer Characteristics 162

Mr. I. Korobkin, Chief
Aerophysics Division, NOL

Special After-Luncheon Address 174

Dr. Wernher von Braun, Director
Development Operations Division
Army Ballistic Missile Agency

- - - - - - - -

Afternoon Chairman:

Dr. R. E. Wilson
Aeroballistics Program Chief, NOL

Comments on Phenomena of High-Speed Impact 200

Dr. A. C. Charters, Chief
Hypervelocity Ballistics Range Branch
Ames Research Center, NASA

Millisecond Measurement of Forces and Moments
in Hypersonic Flow 222

Dr. A. E. Seigel, Chief
Gas Dynamics Division, NOL

Page

Mathematical Aspects of Supersonic Nozzle Design 244

Dr. W. R. Thickstun, Chief
Mathematical Analysis Division, NOL

APPENDIX A 256

Roster of Persons who Registered at
NOL for the Ceremonies

APPENDIX B 265

Program Details for Dedication-Decennial
Affair, 25 - 26 May 1959

APPENDIX C 268

Tour Plan and Map of Aeroballistic Area

APPENDIX D 270

Facsimile of Formal Invitation Card
and Request for Registration Card

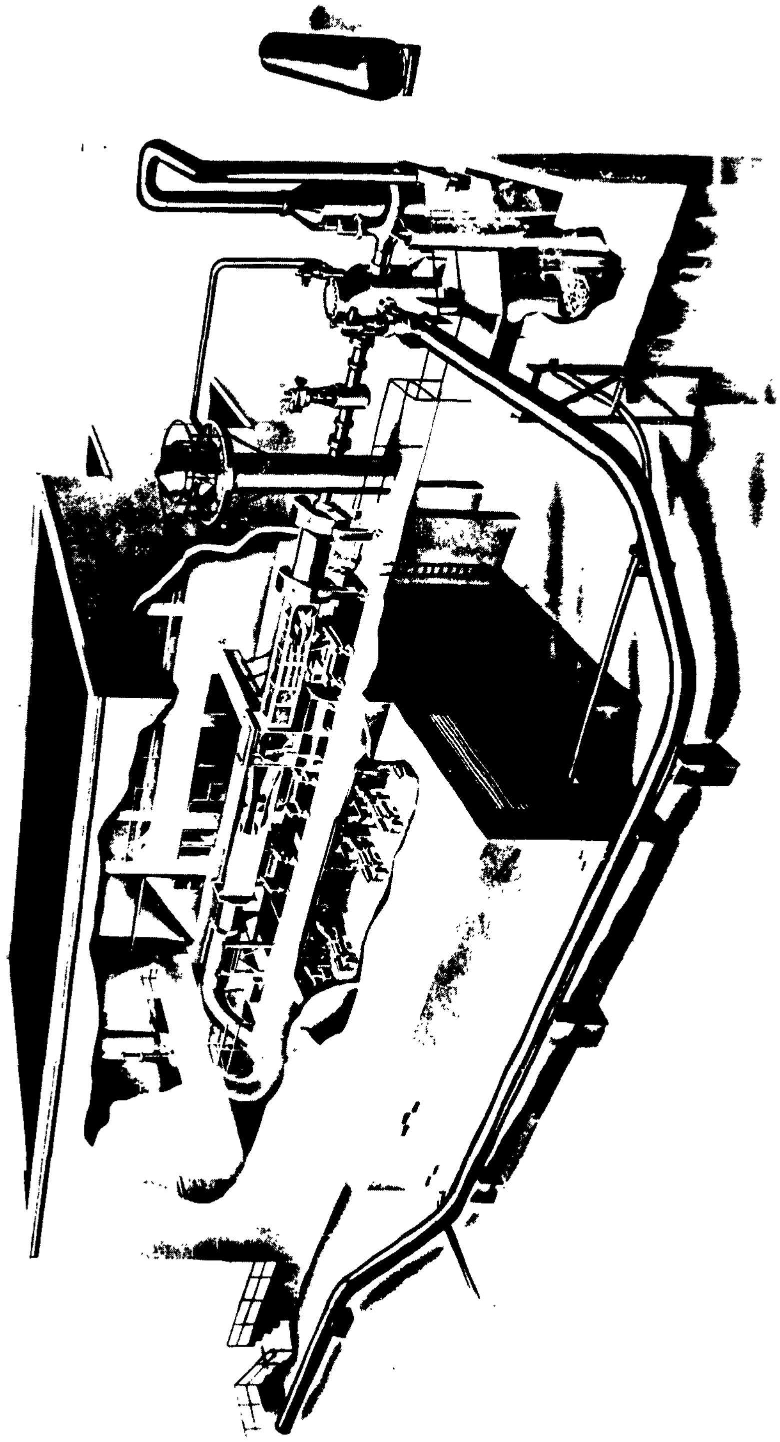
Description and Characteristics

of the

THREE MAJOR NEW

AEROBALLISTIC RESEARCH FACILITIES

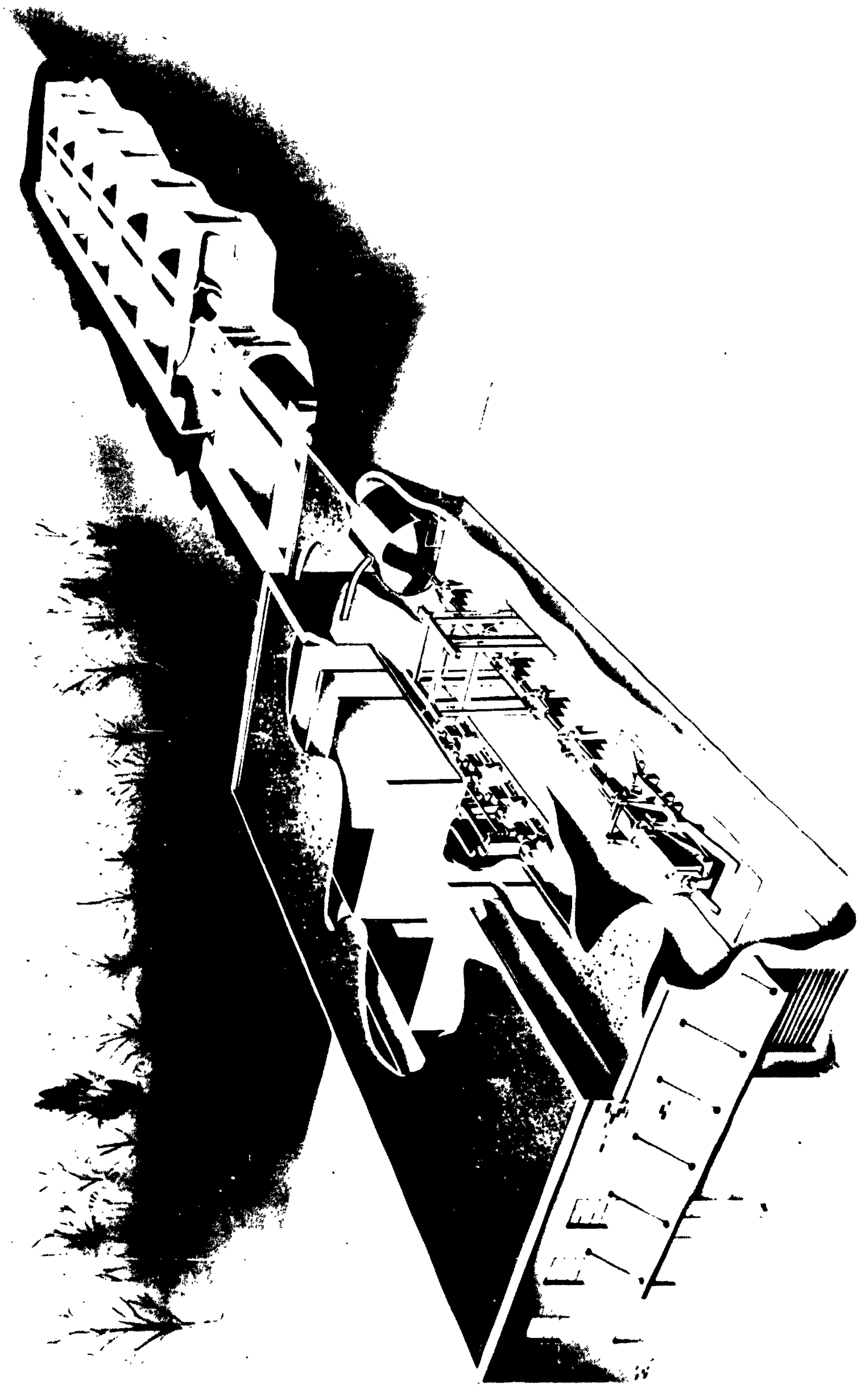
Dedicated, 25 May 1959



This tunnel was designed to provide a developmental testing facility in the hypersonic speed ranges. The tunnel is extremely versatile both mechanically and aerodynamically. It is well-suited for force and moment investigations, pressure distributions, and heat-transfer studies.

This tunnel has the following characteristics:

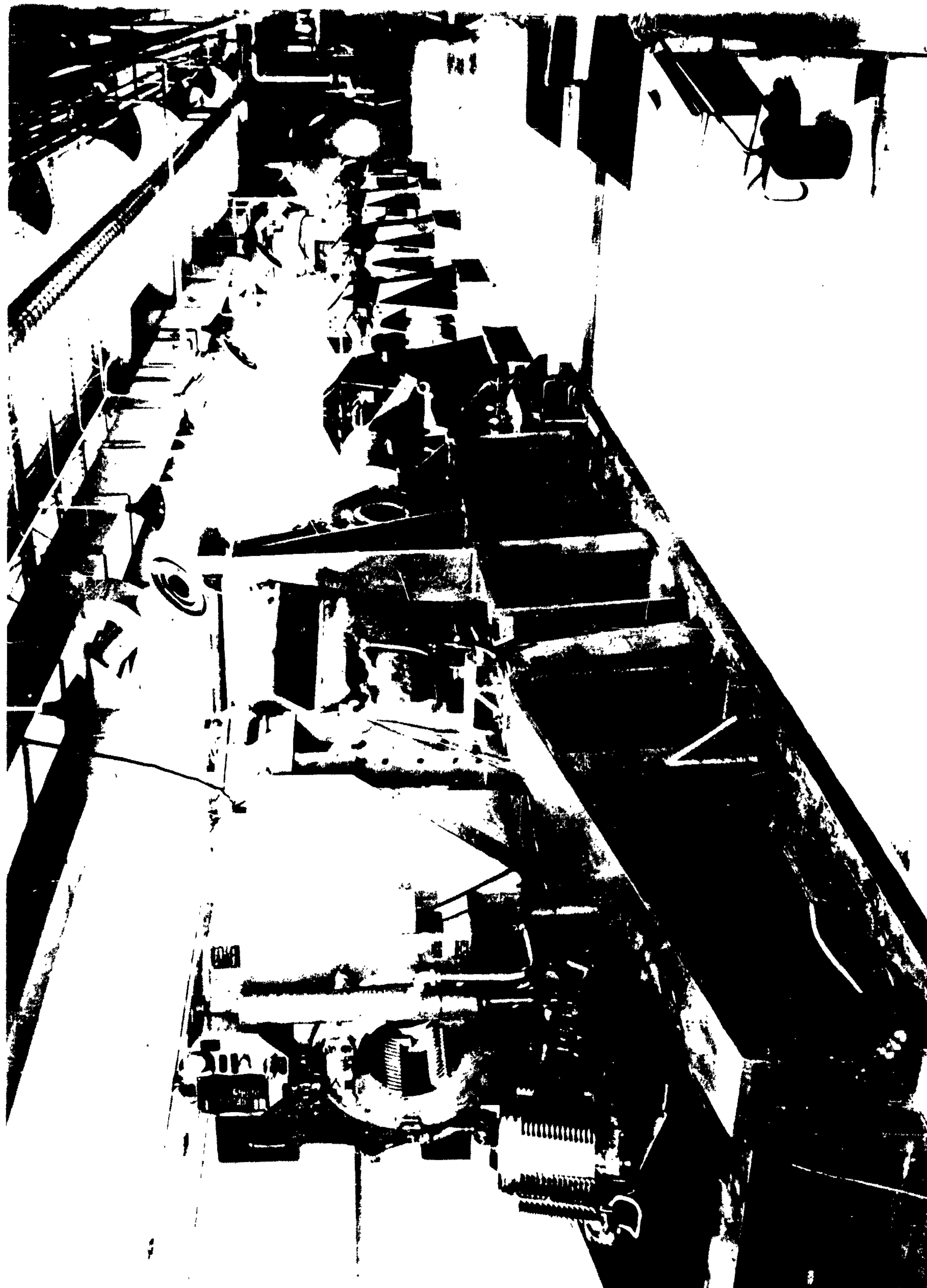
Size:	20 x 20 in. (51 x 51 cm).
Type:	Blowdown (open or closed jet in test chamber).
Mach Number Range:	5 - 10
Blowing Time:	Dependent on Mach number and supply pressure - 1.4 minutes to several hours.
Supply Temperature:	160°F - 1500°F (71°C - 816°C).
Supply Pressure:	1 - 150 atmospheres.
Model Size:	Dependent on Mach number - up to 60 in. (152 cm) long and 10 in. (25 cm) diameter.



This range will be used to study drag, stability, heat transfer, and flow conditions for the development of ballistic missiles and high-speed aerodynamic vehicles.

This range has the following characteristics:

Length:	1000 ft. (304 m).
Cross Section:	10 ft. (3 m) with usable dispersion area of 2 x 2 ft. (0.6 x 0.6 m).
Pressure:	1 atmosphere to 1/1000th of an atmosphere.
Temperature:	$75^{o}F \pm 3^{o}F$ ($24^{o}C \pm 1.6^{o}C$).
Maximum Projectile Size:	4 in. (10 cm) diameter.
Velocity Range:	Above 5000 ft/sec.
Photographic Spark Stations:	6 schlieren, 15 movable shadowgraph (additional stations being provided).



This shock tunnel has an unrestricted nozzle at the end of the supply tube. It is used to measure forces and moments as well as pressure and temperature distributions, flow characteristics, and real gas effects in the development of ballistic missiles traveling at high speeds.

This shock tunnel has the following characteristics:

Size (supply tube):	4 in. (102 mm) diameter, 732 in. (19 m) long.
Size (high-pressure chamber):	110 in. (244 mm) diameter, 144 in. (3.6 m) long.
Type:	Blowdown.
Mach Number Range:	6 to 12.
Blowing Time:	3 milliseconds - 1 millisecond.
Combustion Temperature:	4600°F (2540°C).
Combustion Pressure:	25,000 psi.
Equivalent Supply Temperature:	2400°F - 15,000°F (1316°C - 8316°C).
Equivalent Supply Pressure:	5000 atmospheres - 2600 atmospheres.
Reynolds Number Per Foot:	
Mach 6	$4 \times 10^7 - 1 \times 10^7$.
Mach 12	$5 \times 10^6 - 1.2 \times 10^6$.
Model Size:	Up to 18-in. diameter.

DEDICATION CEREMONY

Monday, 25 May 1959

WELCOME

by

Captain Mell A. Peterson
Commander, U. S. Naval Ordnance Laboratory

Ladies and gentlemen: The Naval Ordnance Laboratory is honored to have you here. On behalf of all personnel at NOL, White Oak, we bid you welcome.

As you leave NOL this afternoon or after the dinner tonight, you will find available a detailed brochure on the Aeroballistic Facilities and a summary of all of the NOL facilities. If there are any that you would like to see would you please give a note to one of the girls in the foyer and an appropriate escort will be provided.

This is a proud day for NOL, White Oak. The facilities to be dedicated today have been obtained because of the active support of the Chief of the Bureau of Ordnance, first Admiral Schoeffel, Admiral Withington, and then his successor Admiral Stroop. We are also grateful for the support of Admiral Raborn, the Director of Special Projects. These were influenced, shall we say, by three Assistant Chiefs of the Bureau for Research and Development who were Rear Admiral Charlie Bergin, Captain Ed Hooper and the present Assistant Chief, Captain Count Ruckner. Also all of this could not have been possible without the constructive assistance of the District Public Works Officer of the Potomac River Naval Command which is now commanded by Admiral Mendenhall who succeeded Admiral Harold Baker.

We are fortunate to have with us today a former commander of the Naval Ordnance Laboratory. You are well acquainted with his accomplishments and his successes. I take pleasure in troducing to you the recently appointed Deputy Chief of Naval Operations for Research and Development, Vice Admiral John T. (Chick) Hayward: Admiral Hayward.

PRESENTATION OF NEW AEROBALLISTIC RESEARCH FACILITIES

by

Vice Admiral John T. Hayward
Deputy Chief of Naval Operations for Development

It is a great pleasure to be here today, and an honor to dedicate these new facilities. I well remember when I was here we were engaged in the struggle to obtain them, and it would be wrong for me to say that it wasn't a struggle. I am sure you will be impressed with the facility when you see it. Of course, you and I know that it is not the facility that will get things done, but it's the people, and the Navy is particularly fortunate in having scientists such as Dr. Kurzweg to lead such groups as we have here at NOL. This relatively small number of scientists and engineers, about two hundred I believe, in the last fourteen years has brought to the forefront the Aeroballistic Research Facility here at NOL, not only by virtue of the work which they have done in the past, but in the actual design of the facilities and the scientific tools they have on hand.

This new facility, of course, consists of a Mach 10 Hypersonic Wind Tunnel with up to 24-inch test section, a new pressure-controlled 1000-ft. Hyperballistics Range (10 feet in diameter), and a 4-inch diameter Mach 12 Hypersonic Shock Tunnel. These represent some of the finest aeroballistic research equipment in the entire world. It would be wrong if I didn't recount some of the struggles to get these facilities because in such struggles there always seems to be a significant turning point. We are fortunate to have here today the man who I say brought about the turning point in getting these facilities, and that was Admiral Raborn, when the Polaris Project came along.

This is an example, really, and it is something that I have learned over the last two years as the Assistant Chief of Naval Operations for Research and Development. We were inclined to have been carried away in the past with the systems approach to weapon development and not to have completed our homework. It is quite apparent that if you look at some of our big missile systems, we put together a system plan and then attempted to invent on schedule. I don't have to name them for you, because there are some that have gone on as long as eleven years, twelve years, that are still not operational today. The real reason is that the component work, the basic work, the work that is long and hard, and the state-of-the-art work had not been done, whereas everybody talks about lead time and lead time decision. The real truth of the matter is that if you had the components and had done the basic research, you could put together your system in a reasonable time. The man who makes the decision as to where he will go within the state-of-the-art is the man who really dictates the time.

As a real simple example, of course, let's take the airplane. If you want a Mach 2 fighter, you can get it today; but if you took the state-of-the-art and said you want a Mach 8 fighter, it would be a good many years before you got it. This is a very over-simplified approach to it, but it shows there are any number of cases such as this where we have picked too far up the curve of the state-of-the-art and, as a result, have not gotten the system into operation when it should have gotten into operation and then it is too little and too late and the system is cancelled. When this happens in our government and in the Department of Defense, you will usually find that the components within the current state-of-the-art had not been correctly assessed.

We in the Navy have just finished taking a very critical look at our organization and our work, and have taken steps to streamline it, and to raise higher the stature of research and development in the Office of the Chief of Naval Operations. It has been apparent for some time that this should be done, and I am sure that it is a step in the right direction.

It is no news to you, of course, that the Navy is no longer master of its own fate. The Reorganization Bill, the Director of Research and Engineering, as well as all of the other changes in organization made it important for us to streamline and make a more efficient setup for research and development. It is most appropriate in that this town lives in, what I call, a fiscal fishbowl.

Now in this reorganization we are putting the Bureau of Ordnance and the Bureau of Aeronautics together. I don't really think there will be too great problems in doing this; I think it is a step in the right direction. It is absolutely necessary in doing this that we keep the technical approach of the Bureau of Ordnance in the forefront. There is no doubt in my mind and in a lot of people's minds that the Bureau of Ordnance has handled the technical and scientific side of life as well, if not better, than anybody in the Navy.

Now we feel that at the Naval Ordnance Laboratory the sort of work that is done at the Aeroballistic Facilities is a must, has to continue, and it has to continue at a very high level. We in the Navy divide our program in the budget side of the book into three parts. Part I is the systems. Part II covers two subjects: (a) that which is exploratory development and component work, and (b) that which is basic and applied supporting research for the support of such places as NOL. It is my plan, and the Chief of Naval Operations, Admiral Burke, agrees 100%, that at least 60% of our budget should go into the Part II side of the house or go into what I call the seed core. That really represents what the Aeroballistic Facility is doing here, and I am sure from the past results that we can expect great results in the future.

With these facilities and this type of people working for us, I am convinced that we will be able to keep the Navy in the forefront doing the job that it has to do in the free alliance. Never forget that we are allied to 45 nations across all the seas of the world, and that we have to control these seas if the alliance is to continue. It is on the basis of this type of work, technical and engineering progress, that we are going to be able to surmount the problems in the future to keep this control.

I want to thank Captain Peterson, Dr. Hartmann, all my old friends out here, for the privilege of coming out and starting what I'm sure will be a very successful symposium. Thank you very much.

ADDRESS

by

W. R. Raborn
Rear Admiral, U. S. Navy
Director, Special Projects

Thank you Doctor. Captain Peterson, Admiral Hayward, distinguished guests, I am particularly happy to be here this morning because some three years ago, when we looked into what was needed for the Polaris program, it was quite evident that a facility of the kind that we are here to dedicate today was lacking in the national scene. Captain Hayward was the skipper at NOL at that time, and on his advice and on the advice of my own technical director, Captain Levering Smith, U. S. Navy, who is also here today, we were very happy to capitalize on the knowledge which is resident here in Dr. Kurzweg and in the others who make up this fine Laboratory. I certainly want to wish Dr. Kurzweg and his fine team every success, and we will be looking forward to getting considerable interest from this investment.

This facility, of course, should do much to contribute to the proving out of the re-entry problem which is present in long-range ballistic missiles. The last twenty miles of its flight is probably the most important, and I am looking forward confidently to the time when we can do away with some of the theory and substitute facts gained from this ballistics range.

I don't plan to touch on the many technical subjects which are connected with the fleet ballistic missile program. Most of you know, of course, the purpose of this program, but I would like to say that it is essential to our security that we develop a dependable and accurate weapons system of this kind. We have become, in the past two years, quite knowledgeable as to what this country needs in the way of major war deterrent weapon systems, and those of us who try to be impartial invariably wind up with the fact that the Polaris missile system aboard a nuclear-powered submarine fills a gap which no other weapon system can possibly supply. This gap needs to be filled at the earliest date.

The club that the Polaris weapon system represents certainly could present certain suicide to any leadership callous enough to launch an unprovoked attack on this country. The built-in early warning system that Polaris supplies is very simple. This system cannot be zeroed in for simultaneous destruction by any foreign weapon system, any attack piecemeal on any of these submarines is, in itself, an early warning system. We don't have to spend millions of dollars in order to get one. We would certainly have any potential aggressor over the barrel because he would be marked for inevitable destruction if he started something. This is an element which as far as I know is unique in the Polaris, major war deterrent system. All the rest of them have been described as sitting at the end of a 30-minute loaded gun. And when you think that you have to be able to decide to fire or not to fire, based on a very short period of time such as 30 minutes, and even if you do fire, it probably will not stop any incoming missiles - and so there could be mutual destruction. This is what I mean when I talk about time for mature decision making processes; and this submarine beneath the water, unobserved and unknown to the enemy, certainly allows us time for mature decision. This is something that is not mentioned too often. Therefore, I believe that whatever mix of major war deterrent systems this country chooses to build, we will find a place for the Polaris-submarine combination. We don't tout it as a panacea; we are probably unique in that; we don't believe that this is the only thing we should have - and I would like some of my friends to be as charitable.

The Naval Ordnance Laboratory will perform a very specific service in an area which can't be seen through a microscope, a stroboscope, or a telescope. It will be your responsibility to devise a means for finding out more about these hidden areas by getting closer with the instruments which you have in this range than has ever been done before. It will provide a unique service to all major weapon programs employing re-entry, and also for space. This is a unique facility and I think this country, not only the Navy, not only the Department of Defense, but this country, can be proud that we have such a facility.

Now as scientists, and technicians, and military men, our objectives are, of course, to provide weapons which will undertake the role of defending this country. And this is a legitimate goal.

I made a speech at the President's church the other night. I did so because the leader of this young men's group, pardon me, young men and women's group, is an officer on my staff. I felt obligated to go. I was very glad I did go, and one of the things that impressed me was the bright-eyed enthusiasm with which these young people of twenty to twenty-five years of age have for this world. They are not cynical like some of us unfortunately have become, and it is a real treat for me to meet with these people and to see the confidence which they have in this country of ours But I would just like to say with respect to this confidence, that some day we hope to be able to divert the very large amount of manpower and resources which we are now putting into national defense capabilities. If we could just get to the place where we could channel a larger portion of this effort into more productive areas then we will certainly have a better place to live. And I, for one, would like to vote for that kind of progress.

The fine group out here at NOL has contributed markedly in other areas, not only to our weapons system but to the weapons systems of all the three services. It is not generally known but the Naval Ordnance Laboratory here has active contracts for both the Army as well as the Air Force, and they are doing a very fine job I am assured by the people who pay the bills.

Thank you for allowing me to come to this auspicious occasion.

RESPONSE

by

Dr. G. K. Hartmann
Technical Director, U. S. Naval Ordnance Laboratory

Admiral Hayward, Admiral Raborn, distinguished guests and members of the staff:

It is a signal honor and privilege to accept these splendid and unique new facilities for the Naval Ordnance Laboratory. We are grateful to be provided with such tools, proud to have accomplished their design and construction, and determined to put them to optimum use for the advancement of science and of defense. For it is not only bricks, mortar and steel that we are dedicating today, but also the people, skills and philosophy of operation that must go with them to make them truly productive.

Almost exactly ten years ago the first Aeroballistic Facilities were dedicated at White Oak. This occasion today and the distinguished attendance here are in a sense a tribute to the accomplishments of the Bureau of Ordnance and the Laboratory in fulfilling the promise of that first dedication. And yet, it is interesting to note that at that time, although much was said about the gradual merging of the sciences of aerodynamics and ballistics due to the inevitable requirements for increasing speed, only a few of the specific problems which face us today were forecast in the speeches. While the success of the V-2 rocket had pointed up problems such as stability and heat transfer associated with the total trajectory including re-entry at supersonic speeds, the extrapolation of these phenomena into the hypersonic regime of Mach 15 and higher seemed many years away.

The first known demonstration of hypersonic tunnel air flow free of liquefaction up to Mach 10 at this Laboratory in 1950 and the accompanying solution of hypersonic tunnel design problems were generally regarded by military people as laboratory curiosities having little relation to the real world of aircraft and aircraft ordnance. This was aggravated by the myriad of practical problems then yet to be solved at transonic and low supersonic speeds. In fact, the first justification for the Hypersonic Tunnel No. 8, which you will see this afternoon, was put forward to BuOrd in 1949 based mainly on advancing the science of ballistics and only secondarily on the assumption that objects of military significance would actually travel at such speeds. It was not until 1955 that this project was funded by Congress. Design based on our pilot experience, and construction, required only four years, thus confirming the usual ratio of 3 to 2 for justification time vs. construction time.

The advent of Polaris changed all that. The Navy's role in providing an invulnerable deterrent force by the combination of the nuclear submarine and the ballistic missile, not dreamed of ten years ago, in one giant step had produced requirements which superseded even the Laboratory's advanced position in high speed aerodynamics. Fortunately, ideas and experience derived during the past decade through basic and applied research were available to allow the rapid development of the 1000-foot Hyperballistics Range and the 4-inch Hypersonic Shock Tunnel making use of light gas guns for launcher or driver. The proposal for these facilities was put forward late in 1956 to the Special Projects Office and the Army Ballistic Missile Agency, and funded in March 1957. The Hypersonic Shock Tunnel was operative in April 1958. The first shot in the 1000-foot Range was fired in June 1958. Elapsed time: about 1-1/2 years.

We were extremely fortunate that funds were also available for the modification of space and for the installation of our high-speed computing facility which is so vital for the processing of wind tunnel and range data as well as for the calculation of three dimensional problems associated with high speed flight.

All of these new facilities are a tremendous contribution to the effectiveness of the Naval Ordnance Laboratory as a complete ordnance research and development establishment. The Laboratory operates as a whole technical team and not as a set of isolated institutes. Modern weapon system concepts require the integrated participation of many different specialties and facilities. Nearly all of our weapons fly through the air with some velocity at some stage in their existence. Accordingly, the science of aeroballistics, be it subsonic or hypersonic, plays a vital role in nearly all of our development projects. We believe that these skills can best be directed to the problems of Naval ordnance through a proper balance between research on the one hand and development on the other. In accepting these facilities and the responsibilities which go with them, we are pledged to a continued standard of excellence, and to the goal of service to the Navy.

THE AEROBALLISTIC RESEARCH FACILITIES AT NOL

by

Dr. H. H. Kurzweg
Naval Ordnance Laboratory

The aerodynamic nucleus which was seeded fourteen years ago at NOL, White Oak, has grown and developed into a most versatile aeroballistic research center; a center which is equipped with modern research and development facilities and with scientists and engineers who are not only scientifically best qualified, but also have a good understanding and feeling for the needs and nature of future Navy ordnance.

During the years, this group has solved numerous aerodynamic, thermodynamic, and ballistic problems by direct or indirect participation in the development of flying vehicles. Furthermore, by carrying out much valuable basic pioneer research work, it has stimulated many new design ideas for short and long-range missiles, bombs, airplanes, projectiles, and also necessary research facilities from which not only the Navy but also the Air Force and the Army have taken their share.

At our first dedication symposium, ten years ago, a first demonstration of the work of the young aeroballistic research center was given. Already at that time a number of good scientific accomplishments could be scored. Most of the plans and ideas, however, were still in the incubation state. What became of them and how they were changed into new ideas and adapted to new developments will be outlined in the following talk.

The line of thought of our aeroballistic people is clearly reflected in the design and construction of our experimental test facilities. Two-fold was always the purpose of their construction: (1) to check new theoretical concepts and calculations and (2) to furnish the missile designers with direct aerodynamic and ballistic data.

Three principally different categories of tools have been developed and applied in the evolution of the aeroballistic science: wind tunnels, firing ranges, and computing machines. Other facilities with specific names and characteristics such as shocktubes, sleds on tracks, free-flight test vehicles, etc. fall under the concepts of these categories. All three are used in NOL's programs.

From the beginning, we concentrated on facilities for aeroballistic research and development work especially in the supersonic and hypersonic region, because the participation of the Navy in the long-range missile program could be anticipated.

Three supersonic wind tunnels and two firing ranges were dedicated ten years ago; at a time when the computing and evaluation of the test results was done by desk calculators and some analytical work was tediously carried out on card-programmed calculators. Two of the three supersonic wind tunnels then dedicated, the 40 x 40 cm supersonic wind tunnels, were born another decade ago in Germany and parts of them, now called 16-inch tunnels, are still in operation today, Figure 1. Various steps have been undertaken to improve and modernize these facilities. For example, the original nozzle blocks with plaster surfaces have been replaced by nozzle steel boxes that are better theoretically and mechanically designed and manufactured. They give excellent Mach number distribution and are practically free of disturbances from the walls. A typical "before" and "after" picture is given on Figure 2. Instead of the 3-component external balances which occupied a great part of the test section internal balances up to 6 components some of which are not much larger than a fountain pen are now used. The third, 7-inch wind tunnel has been retired after the basic diffuser investigations for which it was mainly built were concluded.

The cry for higher Reynolds numbers could partly be satisfied by converting one of the 40 x 40 cm intermittent test sections into a continuous tunnel with up to three atmospheres supply pressure by reinforcing the old test chamber, by installing a new diffuser, and turbulence-reducing settling chamber, and connecting it to a 10,000 HP compressor plant. The entire test section of this tunnel will be replaced in the near future by a new section which can be operated up to 10 atmospheres supply pressure, Figure 3. With the increasing understanding of the viscous effects in supersonic flow phenomena, the cases where high Reynolds numbers are essential for the test can now clearer be separated from those cases where the Reynolds influence is small. In general, the most economical intermittent and convenient one-atmosphere Tunnel No. 1 with 60 seconds blowing time is still in demand in the Mach range up to 5.

At the beginning of this decade the design and construction of a pilot hypersonic wind tunnel up to Mach 10 was on its way. It was only a small, 5-inch tunnel with up to 900°F supply temperature and 50 atmospheres pressure but excellent results have been obtained with it. The air liquefaction problem, for example, in spite of extensive calculations and scientific speculations was not clear ten years ago. It was immediately attacked after the tunnel was in operation and for the first time it could be shown that hypersonic tunnels cannot count on appreciable supersaturation if liquefaction free flow around models is wanted. In the following years other results of basic research in this tunnel on hypersonic boundary layers, diffusers, and heat transfer stimulated many people in this country and abroad in their own planning for hypersonic facilities. Only last year this tunnel was modified to accomodate some development work for the Polaris and Jupiter missiles. In a record time of a few months a new test section which allows the use of nozzles up to 10-inches diameter and a new heater which yields supply air temperatures up to $1400^{\circ}F$ were designed and installed, Figure 4. This modification had to be made due to the urgency of the programs which required results before the construction of the larger 20-inch Hypersonic Tunnel, which was on its way, could be finished. Now, after ten years of pushing, proposing, designing, modifying, and constructing this Hypersonic Tunnel is finely ready for the shakedown. It was a hard struggle, to get it; but, of course, such long periods have been experienced by practically all people who were building large hypersonic wind tunnels during the last decade.

This Hypersonic Tunnel, Figure 5, which is dedicated today has been designed to operate up to Mach 10 with 150 atmospheres pressure and a maximum of $1800^{\circ}F$ supply air. The lower Mach number is approximately 6, so that the sequence of Mach numbers between the 16-inch supersonic tunnels and the new tunnel shows no gap. With these supply conditions no air liquefaction will occur and the Reynolds numbers will be sufficiently high. Two- and three-dimensional, water-cooled nozzles will be used. Mach numbers 8 and 10 will have rotational symmetric nozzles which are manufactured by a new electroplating process. With the new tunnel on hand and with the smaller hypersonic tunnel in full operation the original 5-inch hypersonic test section has recently been retired and transferred to AGARD whose Training Center in Brussels, Belgium, will install and operate it in its course work.

With the continuously better understanding of the aeroballistic problems, the development of missiles and other hypersonic vehicles made fast progress. Unfortunately, with the solution of one problem always at least two new ones occur and there is no single wind tunnel any more from which all the answers can be obtained. The high stagnation temperatures of missiles with more than 10,000 feet per second causing many thousand degrees stagnation and friction temperature, are even with our newest hypersonic wind tunnels impossible to match. With our hypersonic tunnels we will obtain precision measurements of aerodynamic forces and heat transfer in a flow region where real gas effects are small. The data for extreme high Mach numbers, above 10, at true stagnation temperatures have to be obtained by other facilities, the shocktubes, shock tunnels, and firing ranges. However, these facilities do not replace the conventional wind tunnels. The vehicles fly in a wide speed range, even the fastest ones have to start slowly and if some day people want to return from a satellite trip they must slow down in the atmosphere. Therefore, hypersonic wind tunnels of the type we dedicate today will play their important role in one of the most important phases of the trajectories or guided flight.

To get information on the aeroballistic flow phenomena under extreme conditions NOL went since 1950 in the shocktube exploration. It was clear that the Laboratory shocktubes with moderate operation pressures, which were so useful in studies of shock reflections during the years before, had to be modified if at all useful data for development work could be expected. High stagnation temperatures can be obtained with this shocktube principle only by large pressure ratios between driver gas and driven gas. Unfortunately, the pressure of the driven gas cannot be too low otherwise the Reynolds numbers cannot be obtained sufficiently high in an expansion nozzles at a high Mach number. Therefore the driver gas pressure must be very high. A simple calculation shows, that if a Reynolds number of 10 million per foot at a Mach number of 12 shall be obtained a reservoir pressure of approximately 50,000 psi and a supply temperature of 2500°K must be provided. Such a facility is certainly not a conventional wind tunnel. It is also not a conventional shocktube, it is a regular gun in which one goes to the limit of the material. Figure 6 shows the Reynolds number versus Mach number and the operation region of NOL's shock tunnels. The large 4-inch shock tunnel, Figure 7, is the latest design which uses the combustion chamber of an 8-inch naval gun with a 60-foot long barrel from which the air is freely expanded in an 8-foot diameter test section. Approximately two milliseconds running time can be obtained during which heat transfer and pressure data can be obtained. Recently also force data have been computed from 80 pictures taken during one run of a freely floating model at a speed of 1,000,000 frames per second. The test section is large enough, so that, after a suitable nozzle is designed and installed, heat transfer, for example, on a full-scale Polaris nose can be tested.

The merging of aerodynamics and ballistics has brought about fruitful mutual stimmulation of ideas between aerodynamicists and ballisticians. It became clear that the wind tunnels alone could not solve all the problems. Experiments became necessary which had to be carried out in free flight. To straighten out many discrepancies between wind-tunnel results and field test firings only precision measurements under exactly controlled conditions could give the answer. Drag measurements required a large variation of

Reynolds number of several orders of magnitude which could be obtained in a pressurized firing range without extreme difficulties.

The Pressurized Ballistics Range, Figure 8, dedicated ten years ago but not then in useful operation allows a Reynolds number variation by a factor of thousand by variation of the air density between less than one hundreds and 6 atmospheres. Figure 9 demonstrates very drastically that the drag coefficient for this particular cone-cylinder projectile as function of Mach number scatters all over the map when fired under various pressures, equivalent to field firings in various altitudes. The cross plot on the next figure, Figure 10, shows clearly the Reynolds number effect. It shows clearly the speed regions where the Reynolds number similarity is important and where it is not important at all.

The range technique proved to be very sueful also in the study of real gas effects on hypersonic models. To obtain the necessary high Mach numbers which give in the range automatically true free-flight temperatures, either suitable guns must be available or the shooting has to be made in gases other than air. We went both ways.

NOL people developed high-speed helium guns up to 4-inch bore from which sphere models for aerodynamic testing have been fired in excess of Mach 10. It is not particularly difficult to accelerate smaller, pea-size projectiles with 20,000 ft/sec or higher. However such small bodies are not well-suited for aerodynamic or thermodynamic testing. The 4-inch helium gun will fire large projectiles on which boundary layers, wakes, and other separation phenomena can be photographed to study the mechanism of the viscous flow phenomena. At the present time detonation troubles at the high combustion pressures have to be overcome before regular tests can be scheduled. The principle and the construction of this gun is identical with that of the 4-inch Hypersonic Shock Tunnel.

Of course, the 60-foot long gun was too big to be put in the 300-foot Pressurized Ballistics Range. To obtain equivalent observation times with a gun which fires 3 to 4 times as fast as the old powder guns, a new range had to be designed. Again Polaris and Jupiter were the spark plugs. The needs for thier immediate design brought us moral support and money for the construction of a 1,000-foot pressure controlled range with pressures between 1/1000th and 2 atmospheres. Figure 11 shows this range under construction. It was completed last year with a moderate instrumentation and is still in the shakedown period.

In the meantime, the race for higher speed guns continues. Already a 20-mm, two-stage gun with the same driver gas has been built with which 17,500 ft/sec has been obtained. Using this gun for models other than slugs or spheres creates a sabot problem which has not been solved yet for the high accelerations in such high-speed guns.

The other way of obtaining high Mach numbers has also been followed by NOL people with good success. The picture of the main facilities would not be complete without mentioning the investigations made in gases other than air. A tiny pressure-controlled Aerophysics Range, not larger than a breadbox, was used in which experiments of classical simplicity were made at Mach numbers up to 20 for the study of real gas effects, long before the helium guns were available for this speed range. With a conventional 8-mm powder gun, 8-mm nylon spheres were fired in bromine, chlorine, argon, xenon, etc. According to the different molecular structure of these gases the onset of the real gas effects shows up at different temperatures than in air. This was the range in which such excellent pictures as that on Figure 12 were obtained. From the position of the shockwave vibration, dissociation, and ionization has been quantitatively evaluated.

Summarizing the capability of the new NOL aeroballistic facilities is Figure 13, which shows Mach and Reynolds number range with respect to the zones in which flow regimes are divided. The facilities are designed to operate in the continuum flow region where most of the practical problems are located. But it also shows blank spaces where some of our future effort will be extended.

The last figure, Figure 14, shows the location of the facilities which will be visited today.

It is hoped that this brief summary of our work demonstrated on the growth of our facilities, contributes somewhat to the understanding that the most complicated high-speed vehicles also need complicated tools for their experimental investigation and that there is not a single wind tunnel any more in which similarity in all phases of flight can be attained. As good research people, we are convinced that practically all the basic aerodynamic information can be obtained from ground facilities such as described and that full-scale tests should only be made to confirm the predictions and the experimental results.

At the end of the discussion I want to thank all the participants in our struggle for new knowledge. The knowledge which we need so urgently for the defense of the country. All 200 people in aeroballistics at NOL, and those who are now attached to other agencies, contributed in one way or the other to the success of our work and the facilities. I cannot name them all but without a few key figures in the game the picture would be incomplete. For example, Dr. Seeger, now with the National Science Foundation, who started the ball rolling at the beginning of the decade, Mr. Lyman Fisher who was instrumental in putting the first pieces together, Dr. Wilson who integrates new programs and keeps them going, assisted by Mr. Lightfoot the Assistant Program Chief for Engineering. Dr. Lobb who inspires the work of the Aerodynamics Department, and whose Chief Designer Mr. Geineder guided the design of the new tunnel, the Ballistics Department Chief Dr. Slawsky and his live-wire gun and range designer, Mr. Shepard, and last but not least Dr. Roberts, the Mathematics Chief who as the last in a large range of fighters got the IBM 704 Computer to NOL.

A special thanks also to the Public Works, the Supply Department, and Engineering Services of NOL, without their vigorous help we could not have the dedication today.

We certainly appreciate also the fine work which was done by our prime contractors for the facilities, the American Construction Company, Nordberg Company, Foster-Wheeler, and the C. D. C. Controls, Inc. for the tunnels; the Humphreys and Harding, Inc., the Naval Gun Factory, and the George M. Ewing Company, for the 4-inch Hypersonic Shock Tunnel; the Nicholson Engineering Company and Walling and Associates for the work on the 704 housing.

Of course, we are especially thankful to the people in the Navy's Bureau of Ordnance, in the Special Projects Office of BuOrd, and the Army Ballistic Missile Agency, who gave us not only moral support but the money.

The conclusion of the list of thanks shall be with my immediate foremen in the organization, Dr. Hartmann who always gives a helping hand where needed, his predecessor Dr. Bennett and his right hand Dr. L. H. Rumbaugh under whose reign the Aeroballistic Research Department was founded and Captain Peterson who with all the preceeding Commanders of NOL established the right contact we have with the Navy, the atmosphere in which it is a pleasure to work.

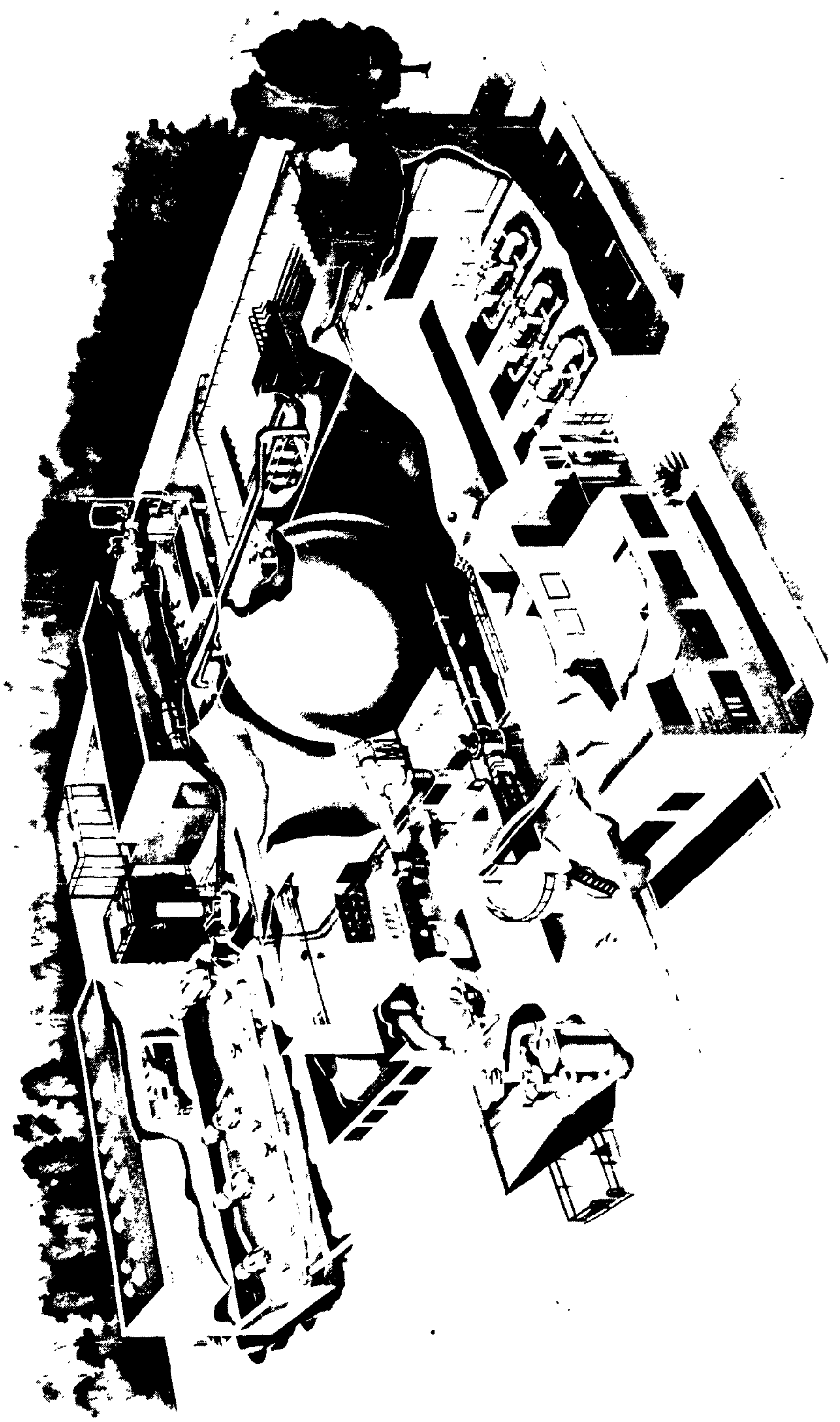


Fig. 1 Wind Tunnel Facilities

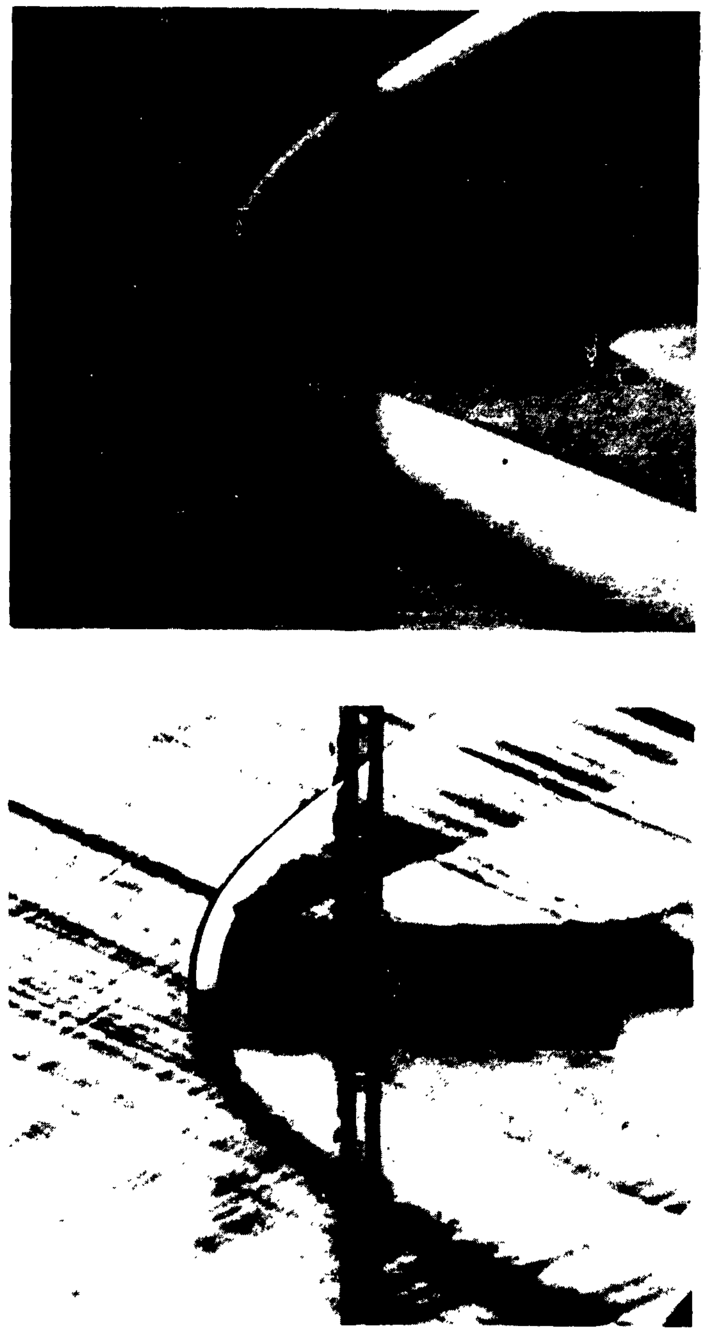


Fig. 2 Wind Tunnel Flow Improvement

Fig. 3 Modernized Supersonic Tunnel No. 2 (Under Design)

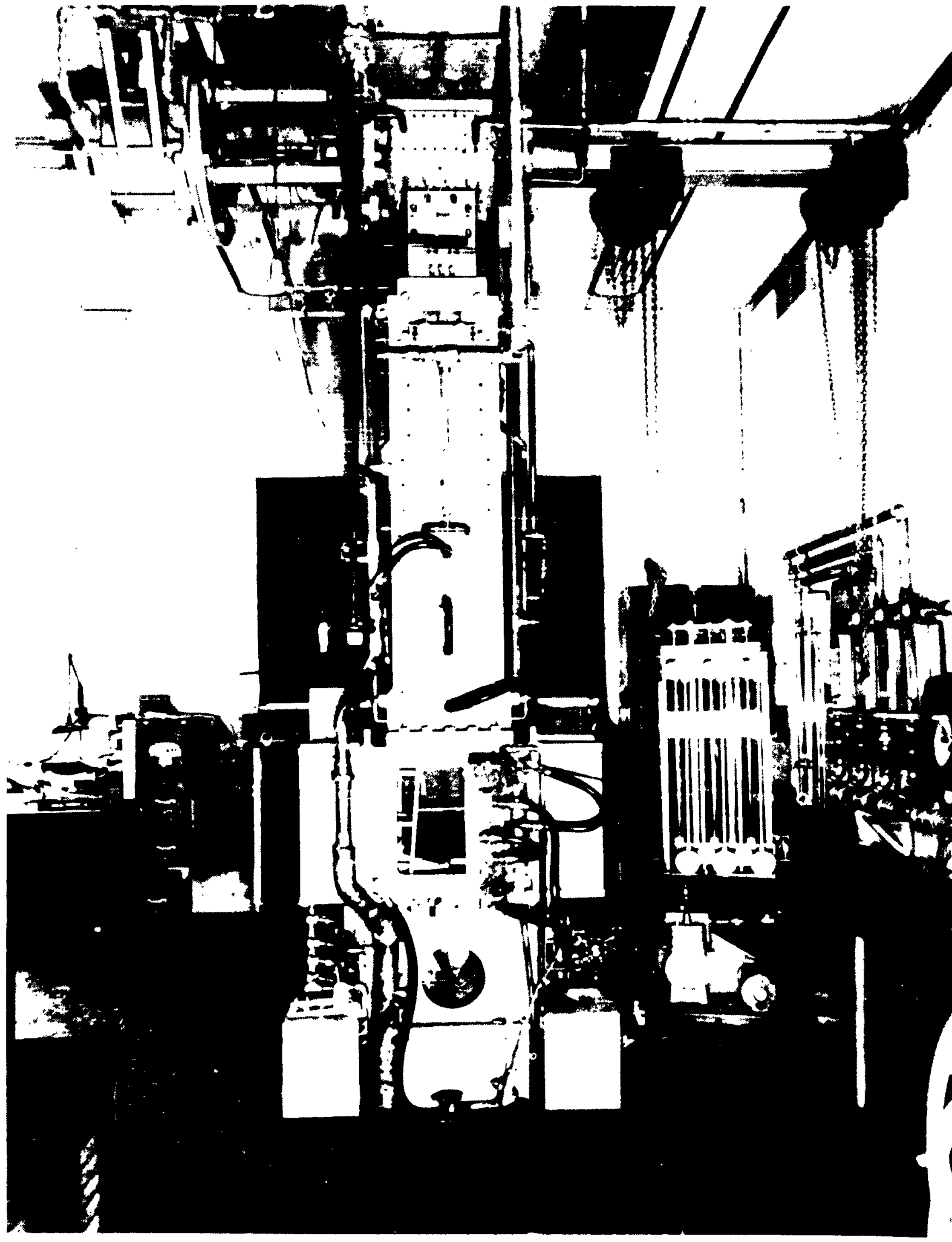


Fig. 4 Hypersonic Tunnel No. 4

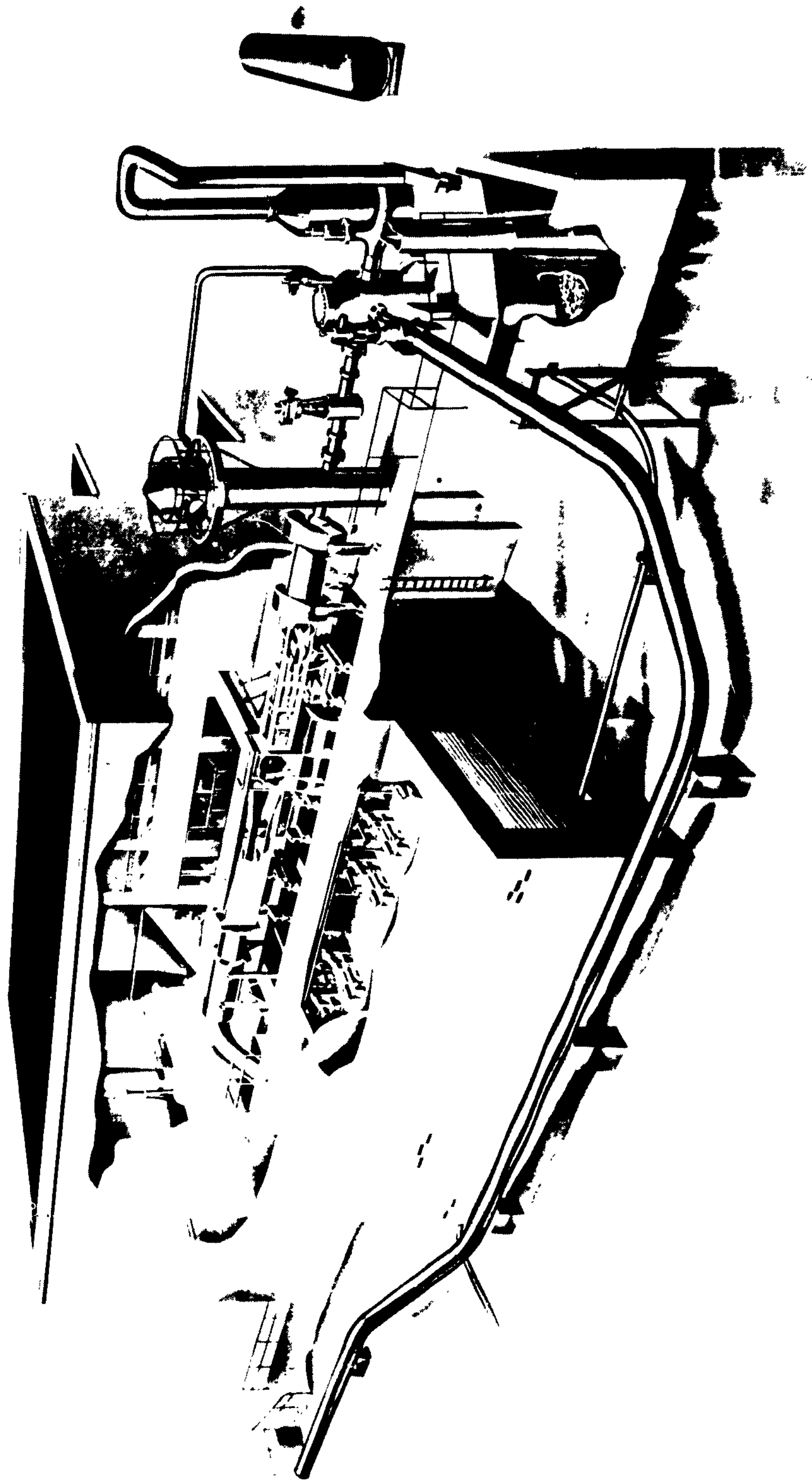


Fig. 5 Hypersonic Tunnel No. 8

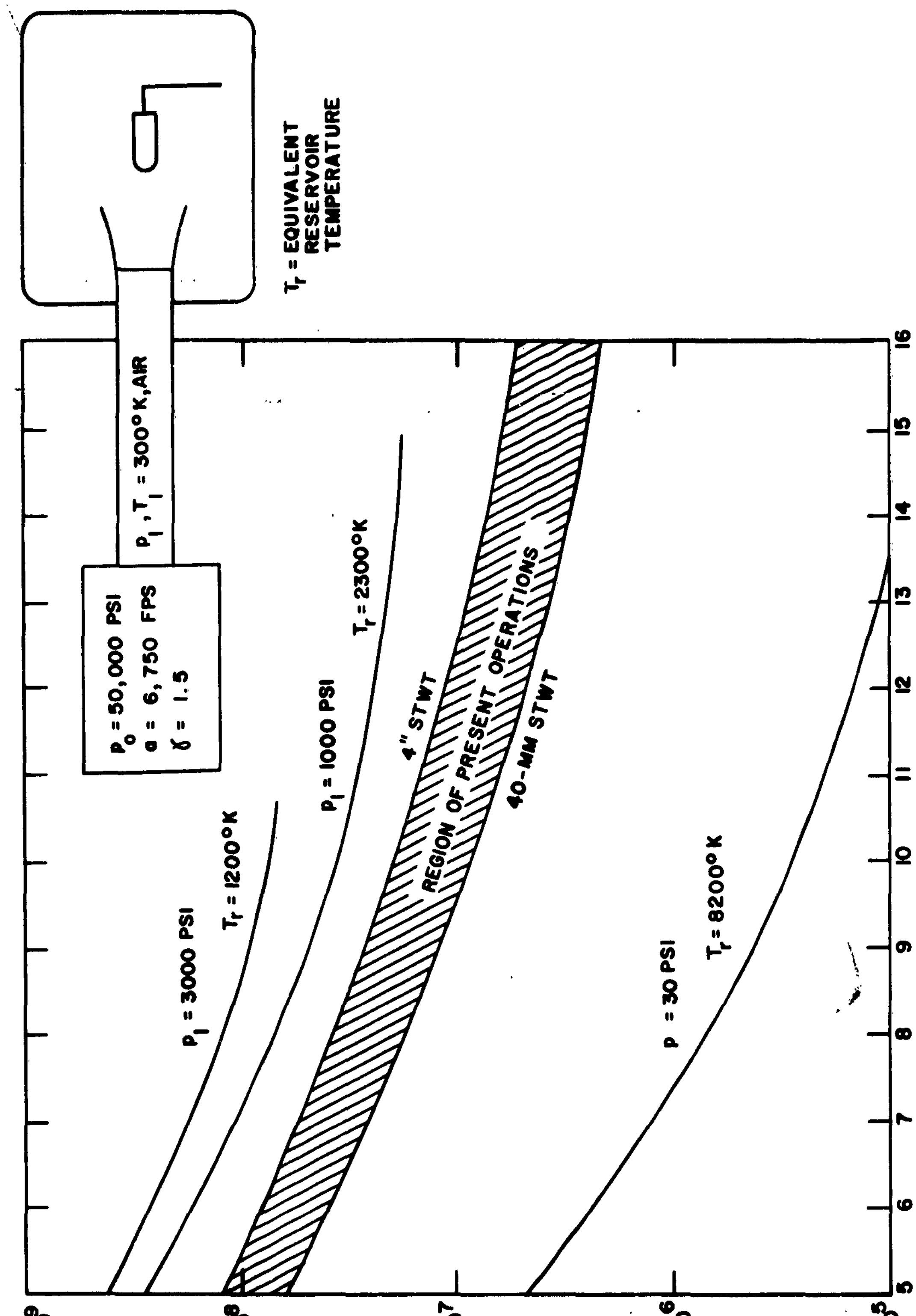


Fig. 6 Operating Region Of Shock Tunnels

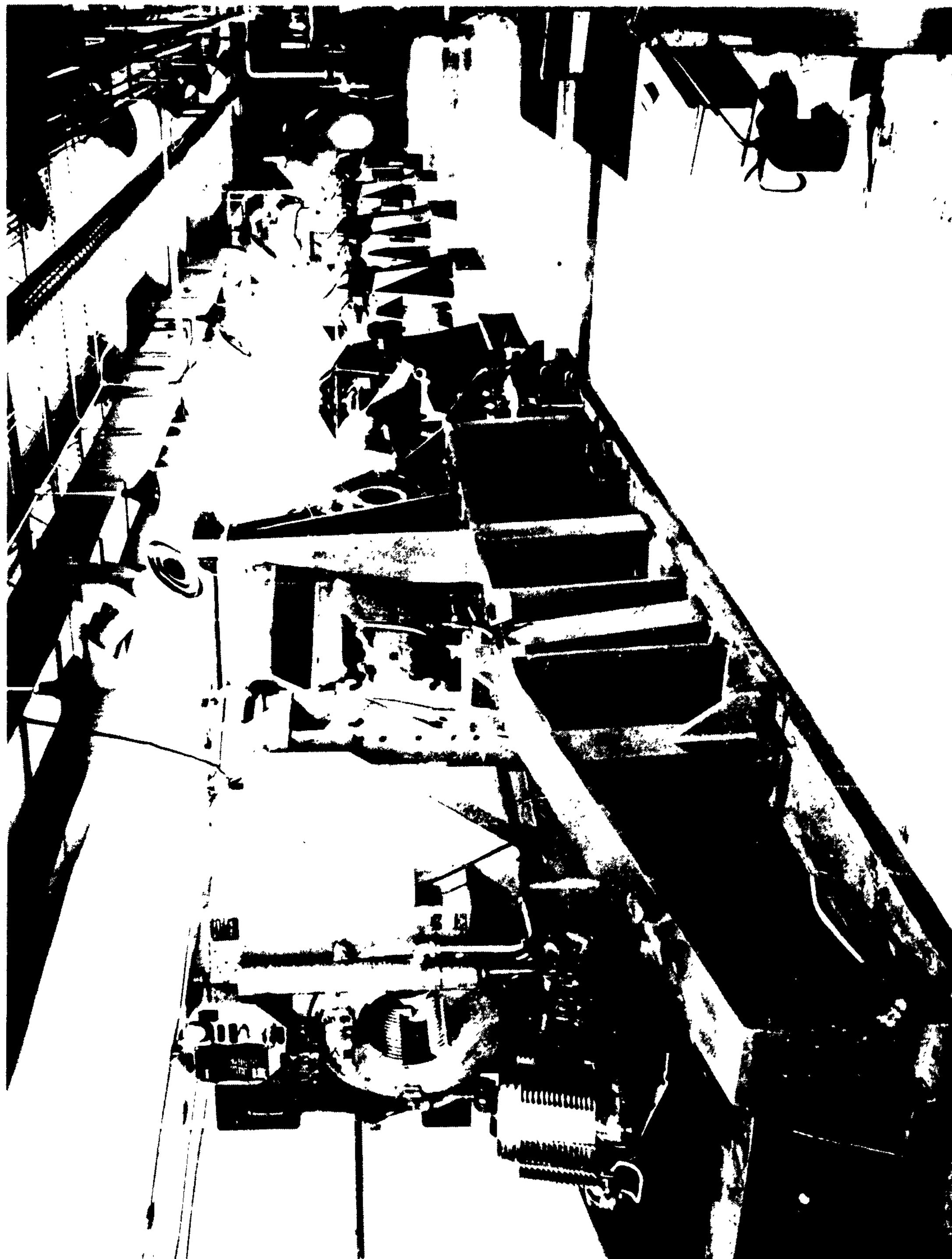


Fig. 7 4-In. Hypersonic Shock Tunnel No. 3

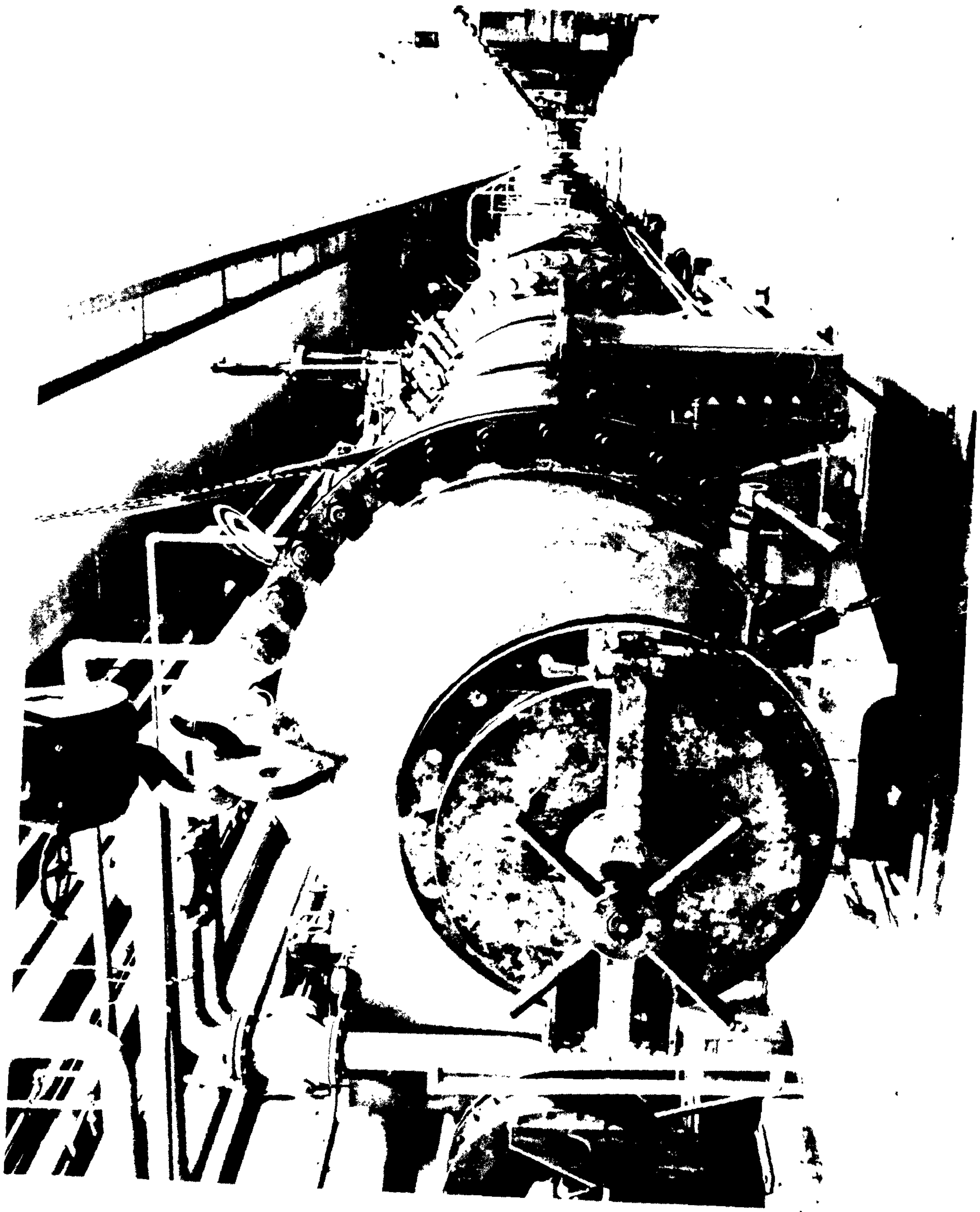


Fig. 8 Pressurized Ballistics Range No. 3

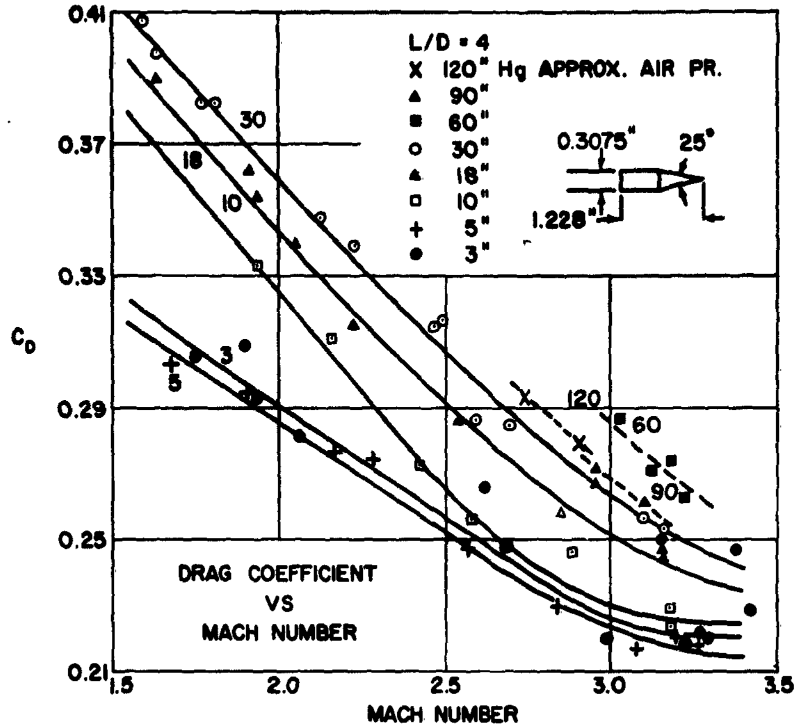


Fig. 9 Drag Coefficient VS Mach Number

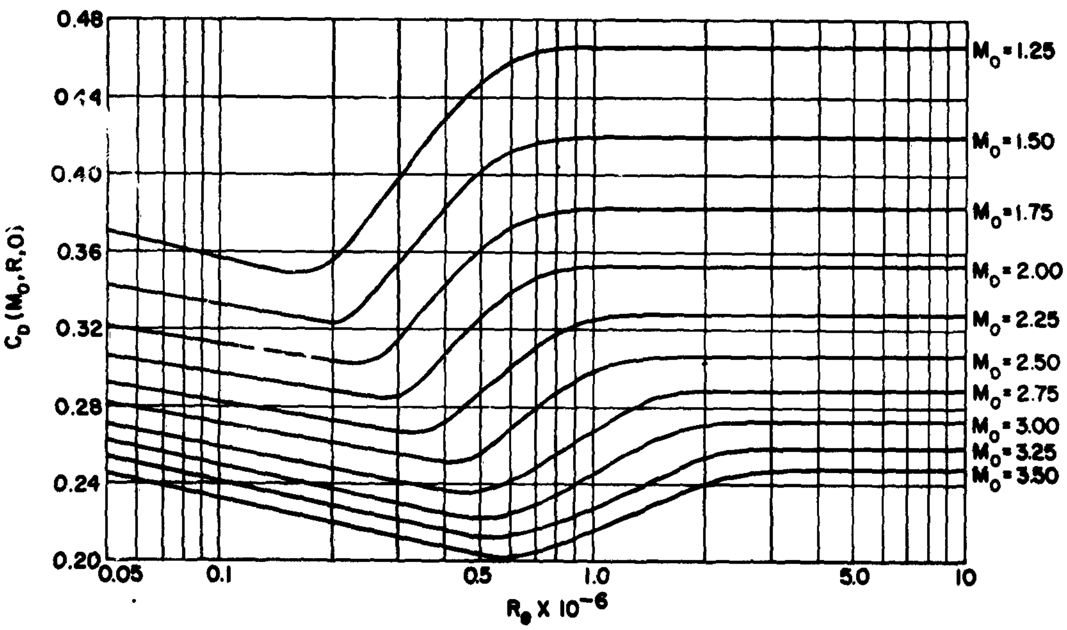


Fig. 10 Cone Cylinders C_D VS R With M As A Parameter

Fig. 11 Construction of 1000-Ft. Hyperballistic Range No. 4

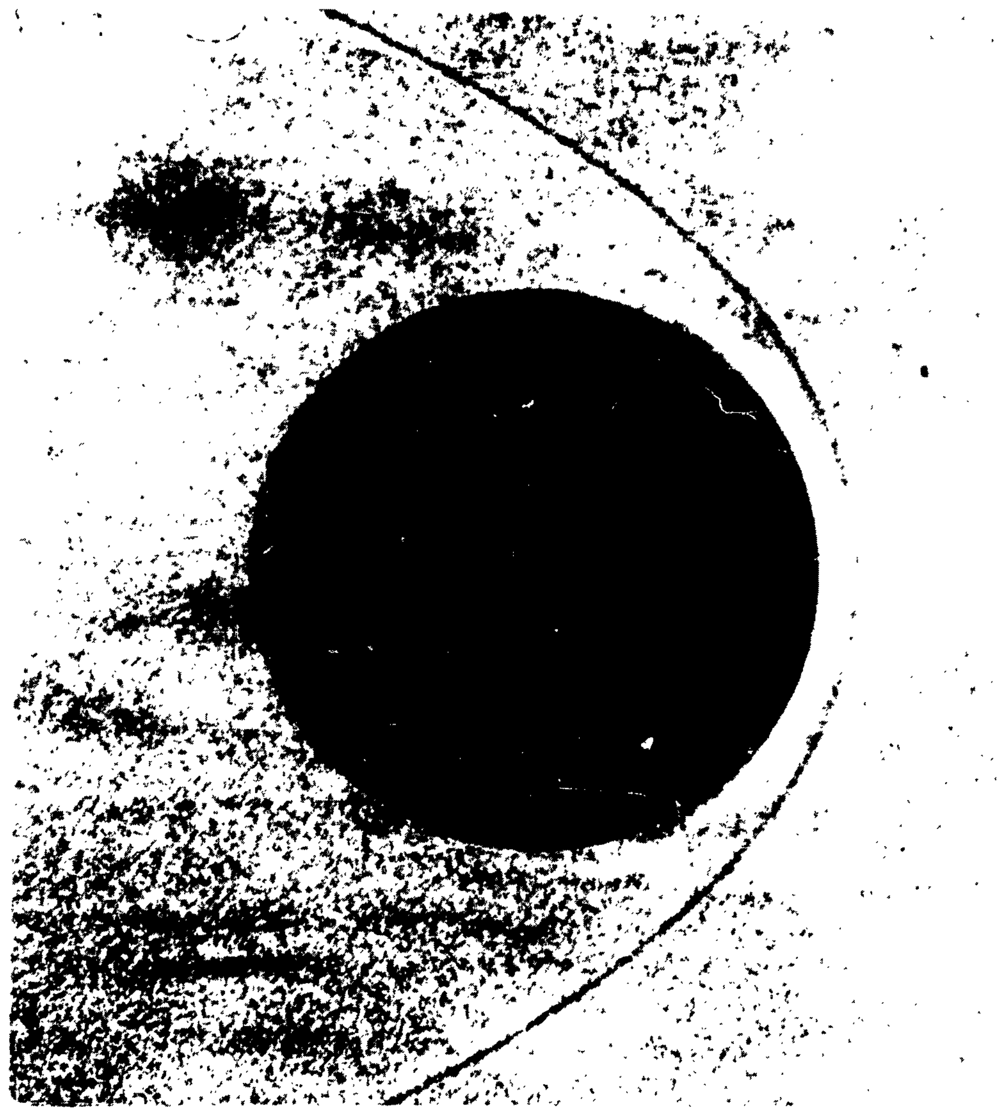


Fig. 12 Aerophysics Range

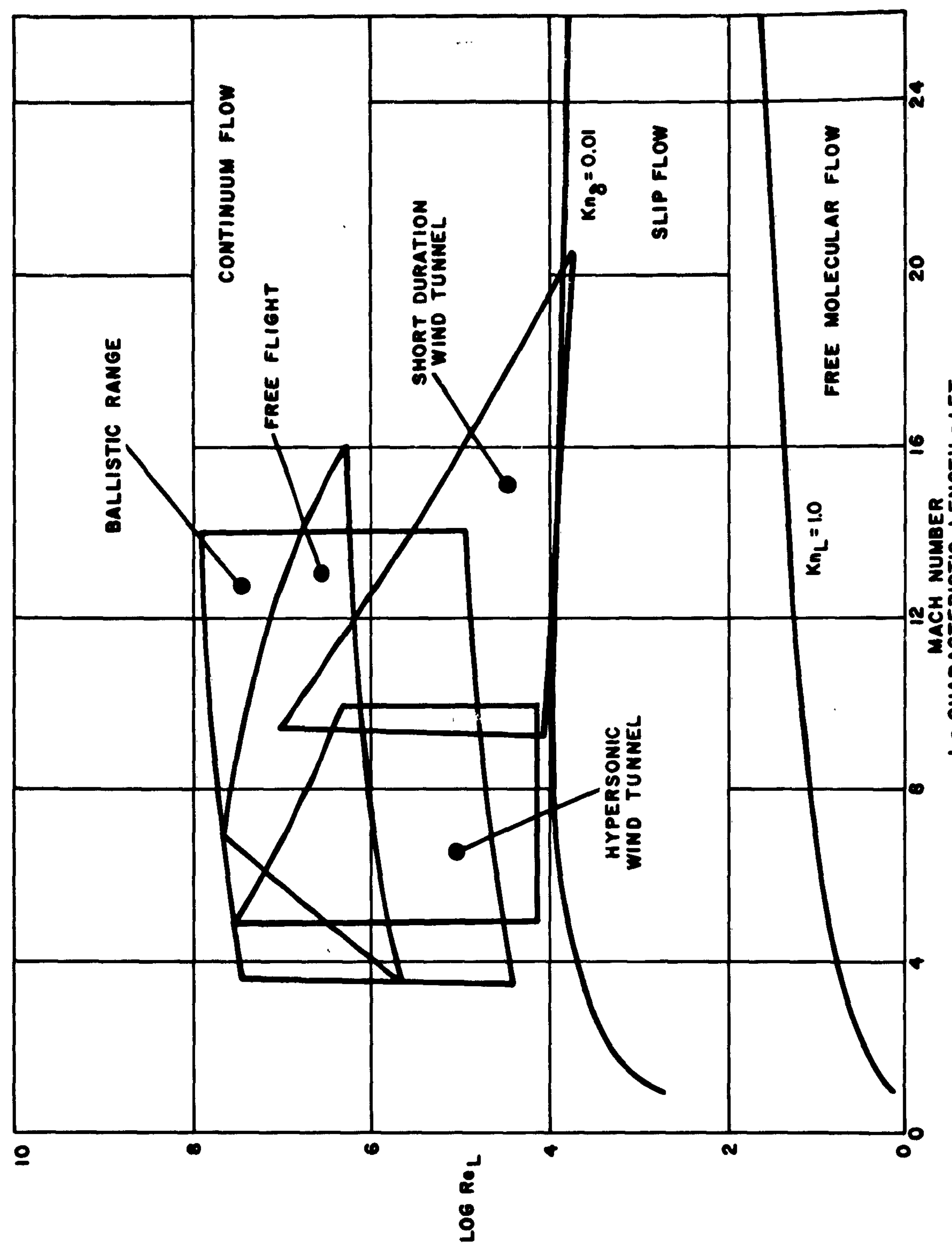


Fig. 13 Operating Regions For Various Facilities

Fig. 14 Aeroballistics Facilities

CONTRIBUTIONS OF AEROBALLISTICS TO SPACE EXPLORATIONS

by
Dr. H. L. Dryden
Deputy Administrator, National Aeronautics and Space Administration

Introduction

Dr. Kurzweg, Captain Peterson, ladies and gentlemen: It is a very great pleasure to bring to you the greetings and congratulations from NASA, from the Administrator, Dr. Glennan, who unfortunately was not able to be present with you this morning, and from the rest of the staff in the field. I believe we have one or two other participants in your program. It is always a joy to see a talented group, under such creative leadership as you have here, receive the tools which are necessary to implement their ideas. These tools are the weapons systems of the war against ignorance. Like military weapons systems, men in the loop are the important elements which govern the usefulness and the output.

Ten years ago I had the honor and pleasure of speaking here at the dedication of the first Aeroballistic Research Facilities at the Naval Ordnance Laboratory. The intervening years have seen many important contributions from the NOL staff made possible by these facilities and which will be reviewed in the Aeroballistic Symposium tomorrow. This last decade has brought a great change in our technological horizons and there is an interesting coincidence which may have occurred to some of you. About eighteen months before the first dedication in May 1949, man first travelled at supersonic speeds in the X1 airplane. The date was October 14, 1947. On October 4, 1957 man launched the first artificial satellite of the earth. We are thus today about eighteen months into the age of satellites. The events are, of course, not quite parallel; but we do not expect the orbiting flight of a man to be long delayed. Speaking for myself, I did not anticipate such progress when I spoke ten years ago.

This is in the nature of a confession. They say that confession is good for the soul. I must confess with many others a certain conservatism which has been tempered within the past few years. Nine years ago I said, not here but at another place, "From this (that is, supersonic flight), it is but a step to consider vehicles which will leave the surface of the earth to become man-made satellites of the earth and a slight additional outreach of the mind to interplanetary travel, or, if that seems too great a step, to travel in space at least as far as the moon. Many competent people feel that a satellite vehicle is perfectly feasible from a purely technical point of view, and I am inclined to agree that the technical problems are solvable with a large but finite amount of manpower and money." If I had stopped there I would have been a prophet; but unfortunately I went on. "I must add that at present I believe there are more important and fruitful ways of spending the money and manpower. I am reasonably sure that the accomplishment of travel to the moon will not occur in my lifetime, and probably not in yours."

By contrast, I appeared last week before a Senate Committee in support of a bill authorizing nearly half a billion dollars for aeronautical and space activities. The bill included continuing support of a project of the highest national priority to place man in a satellite orbit around the earth as a prelude to the further manned exploration of space, projects for scientific measurements in the space environment through the use of satellites and space probes, and projects for applications of satellites to meteorological observations and long distance radio communication. This change in attitude developed slowly. By 1953 I was stating that "We may reasonably suppose that a satellite vehicle is entirely practicable now, and that travel to the moon is obtainable in the next fifty years." Recently, I have been testifying that travel to the moon can be reached within ten to fifteen years if sufficient funds and effort are devoted to the enterprise. There were a few who had greater vision, but the exponential growth of technology resulting from the large support given to the ballistic missile program in the last five years and the demonstration furnished us by our competitor have, I think, opened the eyes of all of us.

Relation of Aeroballistics to Space Exploration

While the launching of earth satellites and planets of the sun is a completely new activity of the human race, it did not appear suddenly as a full blown project. Our current activities are, in effect, an extrapolation of aircraft and missile experience to greater speeds and altitudes. The progression from the Wright Brothers' airplane to space vehicles has been an evolutionary one except for a few "quantum jumps" or "breakthroughs" which turn out to be the result of the convergence of timely developments in a number of related fields rather than in a single bright idea or invention.

Our present intercontinental ballistic missiles were made possible by the maturity of large chemical rockets combined with new developments in structures such as (1) the pressure stabilized thin wall tanks of the Atlas and the integrally re-enforced skin of the Thor, (2) inertial guidance systems, embodying new developments in gyros, (3) light weight nuclear warheads, and (4) new techniques for dealing with aerodynamic heating on re-entry.

For the next decade it appears that the chemical rocket will be the primary propulsion system for space vehicles. These vehicles will be launched from the surface of the earth and in time, certainly when man is aboard, return to earth. As our booster capacity increases we may expect to launch large orbiting space vehicles weighing a hundred tons or more. We may then find it expedient to design and develop entirely new types of vehicles with entirely new types of propulsion, assembling the machines in space and operating them entirely outside the earth's atmosphere; in some cases in regions of very low gravitational forces. At that time a completely new technology will arise unrelated to the extrapolated experience with missiles and aircraft. For such vehicles aerodynamics will be of no interest, and aeroballistics will have no relevance.

However, space exploration will always require vehicles of the types we are beginning to develop, for it will be necessary to resupply space stations from the earth and no matter how far he travels into space man will always wish to return to the earth to bring back his treasure and to tell his fellow men about his exploits. To return to earth, or to land on another planet which has an atmosphere is the most difficult part of the journey. Whether by retro-rocket or by aerodynamic braking, the vehicle must be slowed down from its very high speed to land safety at practically zero speed. The contributions of aeroballistics are to this aspect of space exploration, a safe return to earth.

Aeroballistic Techniques

Now again, confession is good for the soul. This is as far as I wrote out this talk - the rest of it is from a few notes.

Dr. Kurzweg has reviewed for you in some detail the techniques of aeroballistic research. I will skim through these rather hurriedly and in an entirely qualitative manner. I don't believe you'll hear me mention a pressure or temperature or anything of this sort, but just to give in one place a complete view and to make one point.

As Dr. Kurzweg mentioned, we start off with subsonic flow phenomena and find that we have to obtain results at the correct Reynolds number if we wish to obtain results on models that will compare favorably with full scale. As we go to the region of supersonic flow we find that, as you have seen in a beautiful demonstration arising from the work of this laboratory, we must not only have the correct Reynolds number, but also the right Mach number. As we have gone into hypersonic flow we find that, in addition to having the right Reynolds number and the right Mach number, we must have the right enthalpy of the fluid. We have not yet solved the problem in any single piece of equipment, as has been mentioned, to simultaneously meet all the requirements and therefore a variety of devices have been developed. One of the early and basic ones was the attempt to extend the wind tunnel methods in this field. There are so-called hypersonic wind tunnels. I will say nothing more about them except that we can rather easily simulate the Mach number, with somewhat more difficulty the Reynolds number, but we have very great difficulty in simulating the enthalpy of the fluid for very high Mach numbers. We then have the development of the two other devices, basic devices, that have been mentioned - the shocktube in which it is possible to get the correct enthalpy, and perhaps now all that we desire in the other respects; and the ranges which simulate other aspects of the overall flow. These have been the basic tools that have had a considerable history in the development of aeroballistics.

The point I wish to make is, first of all, that there have been developments in the technical capacities of the tools, and you have heard some of these. We are now not afraid to build a wind tunnel to operate at temperatures of 1800 degrees F. and there are some enterprises underway which will go to still higher temperatures than this. We have developed a whole new variety of devices that we call guns, although I think that the main resemblance to a gun is that they have a barrel. They are devices which sometimes use a shocktube as a part of a process for projecting models at very high speeds. You have heard some discussion of these devices.

The only point I want to make in describing these tools is that we have started along the road of trying to combine in one device two of these three devices and almost all the combinations have appeared. Rather early, as you know, the range was combined with the supersonic wind tunnel. In other words, a gun was used to shoot a projectile in a wind tunnel and a combination of the speed of the projectile and the speed of the air in the wind tunnel, by a little bit of cheating made 2 + 2 equal 6 or something similar. The fact that the temperature at the throat of the wind tunnel was different than that in the air in which the gun was fired enabled you to have a higher Mach number than you could with either device used independently, and greater than the sum of the two. I might mention that there have been refinements of this device in the form of the so-called atmospheric re-entry simulator in which the shape of the wind tunnel is so arranged that the density variation simulates the variation of density in the earth's atmosphere or in the atmosphere of some other planet. This has proven to be a very useful form of that combination.

Dr. Kurzweg mentioned the shocktube wind tunnel, a combination of the shocktube as the generator of flow with a nozzle test section in which a model can be placed. There are underway some proposals for combining the range and the shocktube for creating flow at the right enthalpy for a very short period. If we can manage to shoot the high-speed gun so as to get the model in the correct relation to the shocktube within the few milliseconds of flow, it seems possible to simulate the conditions at very high velocities indeed. Some of my NASA colleagues hope to reach about 50,000 feet per second with such a combination. I am not sure that these represent all of the combinations of three devices, taken two at a time, but I describe these to illustrate the development of instrumentation and techniques. I could, of course, spend more time on describing accompanying developments in instrumentation, as Dr. Kurzweg has mentioned, but I think I have made the point that the tools of research are in a continual state of development. Research needs to be done in this area and particularly we all need to get the money to build some of these tools after we have done the preliminary thinking about them.

No matter how much we do in the way of experimenting, we can never experiment under all the conditions in which we are interested. No experimental program will ever give enough information, and most of you in this room know without my telling you that the experiments become literally orders of magnitude more productive when they are related and correlated with simultaneous research by pencil and paper and computing machine. I suppose that Dr. Kurzweg speaks about mathematics as the third element in his talk because the word theoretical research has acquired an undesirable psychological connotation and perhaps it is a little harder to get the money for this work. I think that mathematics and computing machines again are the tools of theoretical workers as they set themselves to create models of the actual physical world, and seek to compute the consequences of the assumptions made in those models and to compare the results with suitably planned experiments. So that as a part of the technique and part of the tools we must place theoretical research on the same level as experimental research.

There has recently appeared, as perhaps some of you know, a book by Wally Hayes and Ronald Probstein on hypersonic flow theory. I have just received a copy. This started as a 60 page summary for "Advances in Applied Mechanics" that Dr. von Karman and I had persuaded Wally to undertake. Then he got so interested in the subject that the first thing we knew he had a book on hypersonic flow theory. In looking at this book I do find recorded there work of some people of this laboratory, Dr. Korobkin, Dr. Lobb, Dr. Winkler, Mr. Persh and others. This theoretical research is an important part then of the overall picture.

Fields of Research

I wish to close simply by talking a little about the fields of research in which aeroballistics will contribute to space technology. I think it should not be a new thought to you and I don't believe we need to attribute to the Naval Ordnance Laboratory group the motives of everybody now-a-days to ride the space bandwagon. Somehow or other many seem to think there is going to be a lot of money available and therefore would like to be identified with space. I think that basic research, as always, makes contributions to a great many technical fields, and it is impossible to say, when we are carrying out work of the type that has been carried out so successfully here, to what technological field the work will be applied. I feel sure that a great many of the results which come out of this laboratory which may have been originally stimulated and worked on in the atmosphere of the weapons requirements of the Navy will be useful in the space field. The greatest contributions are, of course, to the re-entry problem as I have mentioned, to re-entry aerodynamics. The ranges have been extremely useful tools in dealing with the dynamics of bodies flying through the air at high speeds. We are able to make measurements of a type that cannot be made in the wind tunnels, and the ballistic techniques of hurling things through the air have contributed a great deal to the analysis of motion. This, of course, is not as simple as you think. I will again do a little confessing to you -- everyone who has analyzed the problem of getting a man into orbit around the earth as quickly as possible has come to the conclusion that the simplest procedure is that which we have adopted in Project Mercury, of substituting a manned capsule for the nose cone of a ballistic missile, firing it into orbit. This is a very simple drag type object and that is all there is to it. As we get into the practical development of this vehicle, we sometimes wonder whether it is as simple as has been talked about. There are problems of dynamic stability; merely because a body is symmetrical does not mean that it will fly through the air with its symmetrical axis in the direction of the line of flight. If it is not in the direction of the line of flight there will be lateral forces and there will be moments and there will be angular motions as well as this pure retardation which you think of when you first arrive at the concept. We are finding that it is as necessary to support this type of vehicle with a large amount of wind tunnel work as it is to support the development of any airplane. We are making measurements on configurations of this capsule in a great many wind tunnels in the country, including such wind tunnels as the hot shot wind tunnel of AEDC and a great many of our own wind tunnels, so that in this field of re-entry aerodynamics, stability, forces acting, etc., we find that the aeroballistic tools make a great contribution.

The next important area, of course, is that of re-entry heating. In connection with the study of heat transfer this laboratory and others have done a great deal of work on various schemes for dealing with the heat problem. It is true that in the case of the ballistic missile or the Mercury capsule the problem is of a somewhat different nature than that of a winged vehicle which is exposed to heat for a much longer period of time. In the missile problem or capsule problem we can deal effectively with an integrated total amount of heat and deal with it properly at a high rate during only a part of the trajectory period. In the winged vehicle we are more likely to deal with the problem of heat over a long period and therefore it is necessary to get a very low rate of heat transfer. At this laboratory you have worked on methods of introducing cooler gases into the boundary layer through a porous surface as a means of dealing with the heat problem. In the hypersonic tunnel which we have under construction at the Ames Laboratory, we are using a film of helium as a means of cooling the walls of the nozzle. In the ballistic missile case and in the Mercury case we are trying the heat shield method or heat sink method, simply having a mass of metal of sufficient weight to absorb the heat. We are also using as well the methods now used in ballistic missiles of ablation of a suitable material from the missile surface. Aeroballistics and the tools of aeroballistics and all the mathematical theory and all the rest will find application to space problems in dealing with heating on re-entry.

There is the problem of the overall dynamic effects in which the designer is interested. The designer will do a better job and his advisors will do a better job if they understand the fundamentals of the problem. The study of hypersonic boundary layers and the effect on these boundary layers of mass transfer; the effect of ionization when the enthalpy is very high and the speed is very high, -- all of these contribute to a better understanding of the problems that are met in the design of space craft to return to the earth or to enter the atmosphere of another planet.

Finally, I would just mention that these same tools have been useful for studying impact phenomena. I will not say anything in detail about this since Dr. Charters is going to give a paper tomorrow describing some work in this field and some of the interesting experiments, and speculation at least on the origin of craters on the moon and the reasons for some of the things that we see in photographs of lunar craters.

This is a very brief review, then, of the contributions of aeroballistics to the problem of exploration of space. We in NASA are quite gratified that here is one of the few groups of individuals interested in basic knowledge of the problems of hypersonic flow. We wish them every success, and hope they have many more ideas, and hope that the Navy finds the money to help them try them out.

EVENING AFTER-DINNER TALK

by

Dr. Theodore von Karman
Chairman, Advisory Group for Aeronautical
Research and Development, NATO

Dr. Kurzweg, Captain Peterson, Dr. Hartmann, ladies and gentlemen. I am very glad to be here and I feel it is an honor to give, as they say, the after-dinner speech. An after-dinner speech should really start with a story: It is the Anglo-Saxon tradition. My story goes back to Roman times when the Emperor Nero amused himself in the Colosseum in Rome by throwing Christian martyrs before the lions. Once when he did that one of these Christians went to the lion as the lion came into the cage and whispered something in his ear. After that, the lion would not touch him. The Emperor's men drove on the lion, but still the lion would not touch the Christian. So the Emperor got interested and asked the Christian to come to his lodge. He asked him, "What did you say to the lion?" The Christian answered, "I can tell this to you only if all the Christians are freed." The Emperor said, "All right, all the Christians are free, now tell me, what did you tell to the lion?" The Christian answered, "I said to the lion, I reckon they expect you to say a few words after your dinner!" Now, I cannot imitate the lion because I've had an excellent dinner, and anyway, I am on the program.

Well, there was the problem "What shall I talk about?" I asked Dr. Hartmann, "What shall I talk about?" He said, "You shall talk about 25 minutes." That was some help but not very much.

We are celebrating here an excellent institution which was put into existence with a wonderful outlook for the future, so I thought maybe I would talk about the past, and because my background has been more Air Force than Navy I went to gather some information about Naval Ordnance. In the library of the Cornell University, I found a book which is called "A Treatise on Ordnance Gunnery." Now, I thought, I will really learn what Naval Ordnance is. This book says "The Secretary of the Navy approves the use of this work as a textbook for the Academy". This book was published by the Bureau of Ordnance and Hydrography, July 9, 1859, so that would make it 100 years old.

Well, I started to read this book and was surprised at some of the text. For example, when the author talked about the practice of gunnery, he talked about wooden ships and iron ships, and in one paragraph he said, "Iron vessels are unfit for war purposes". He also said, "From what has been stated it is evident that iron vessels, in spite of their great advantages, are utterly unfit for purposes of war." Well, I don't think that this statement quite represents the present opinion of Naval Ordnance.

Then I became interested in what the author had to say about rockets, and that was even more interesting. He said, for example, under the title "Limited Usefulness of Rockets": "Rockets may be used to some advantage against cavalry because of the scaring effects of the blazing projectile upon horses, also against large masses of infantry, but they are totally inefficient in firing against small objects." Later, as an explanation he said, "When the composition (meaning the propellant) is entirely burned out" (he means the famous burnout point which is today so important in missile technique) "when the composition is entirely burned out the rocket proceeds in a new and very different condition, so that, under the whole, it is utterly impossible to lay down the trajectory of a rocket." This is true! I worked a long time on rockets and must say this is true. "Or," he said, "It is impossible to obtain good and sufficient rules for conducting the practice with that art".

I was somewhat disappointed because I found very little about Aeroballistics -- after all we here are interested in Aeroballistics -- and I think the only thing that I did find on Aeroballistics is a very competent description of the experiment of Magnus. You know that Magnus was a famous professor in Berlin; the author very nicely described Magnus' theoretical investigation on the deviation of projectiles and also the so-called Magnus effect of rotating cylinders. So on this point he was very good in Aeroballistics, but I was somewhat disappointed in the other contents of the book.

Now, since I was already in the historical study, I found in the same library another book called "Ballistics in the 17th Century". There I got a completely different picture. First, the book deals with the beginning of the Renaissance period, the time of Leonardo da Vinci and Galileo. Incidentally, Galileo's last name is Galilei, but the American students in general seem very familiar with him so they call him by his first name. In fact, Galileo was his first name -- his full name was Galileo Galilei. In Europe they call him for the most part Galilei.

In ancient times the theory of the motion of a projectile was quite different from modern concepts. Thus, Aristotle saw the problem not from the point of view of air resistance. He did not ask how much resistance the air offers against a stone or an arrow. On the contrary, he did not understand why the stone is able to move, because apparently no force was present to sustain the motion. For example, it is evident if I want to move this book on the table then I have to push it. The ancient philosophers concluded -- if there is no push there is no motion. So the problem was: Why does the stone, which nobody pushes, really move through the air?

It is very interesting to see through these historical books how ballistics, for example, Aeroballistics, contributed to the development of not only mechanics but also mathematics. First, let us consider the Italian group which started ballistics. The first man you can call a ballistician is a gentleman called Tartaglia. Leonardo da Vinci followed him; he had the right ideas how a bullet or a body could move through the air without a pushing force. He also had the right ideas about the influence of gravitation and the influence of air resistance. The Theorem that a body without action of forces moves uniformly in a straight line we call Newton's principle. But already Galilei knew that a body moves in a straight line at uniform speed if there are no forces against it. So you see, Galilei and Newton took opposite viewpoint from the Greeks. Now the problem is: Why doesn't the bullet go in a straight line to infinity? Galilei recognized that this is due to gravitation so that the weight has an influence, furthermore the air resistance has an influence. In Leonardo's sketches, Galilei's conversations and dialogues, these things are discussed correctly, but not yet mathematically. The Italian ballisticians found that under the action of the gravitation the trajection of a body is a parabola and so they put together the actual trajectory in air as consisting of parabolas. Newton was the first to announce the mathematical fundamentals of mechanics. He shows a drawing which represents the motion of a body, first from ground to ground, then he shows that if the velocity becomes larger and larger you get a satellite; so he was really an aeroballistician of the most modern type. However, the mathematical solution was rather difficult because if you take into account both air resistance and gravitation in a current way, you need calculus. It is interesting to see that the ballistic problem led both Newton and Leibnitz to the development of the concept which they called fluxio, which is identified today with what we call calculus. Also, you remember it was a great discussion between Leibnitz, Newton and Jean Bernoulli not only who discovered the calculus but how one has to formulate it. Jean Bernoulli's letter is full of name calling. It is not at all polite concerning Newton. And, the discussion was always around the ballistics problem.

I think this is enough of history. In fact, I am not very well versed on this subject so I want to leave the discussion of the discovery of calculus to the historians and come back to the present status of ballistics. I would like to say a few words from the viewpoint of the aerodynamicist.

Dr. Dryden talked about space exploration this morning. I find that space exploration is an important thing -- a very interesting thing -- but I don't like it when people say that aerodynamics is now obsolete and space science starts. After all, when we deal with flight with extremely high velocities through high altitudes, where does space start? Nobody knows.

I think the United Nations would perhaps decide that this is a question for the lawyers. I don't want to say anything against lawyers but I remember that in 1912 I participated in what I think was the First Congress for the Law of Air, for "Luftrecht". The Congress took place in Frankfurt, Germany, and we talked about the air space which belongs to a country. The great lawyers worked out a proposal which said you form a cylindrical surface over the frontiers of the country and the air included in this cylindrical surface belongs to the nation. Now, there was an engineer present who said, "Gentlemen, what is between the cylindrical surfaces?" Because it is evident that if you take the State of New York and form a cylindrical surface over its borders, and do the same thing with Connecticut or Massachusetts, between there will be a neutral part which does not belong to any state. Finally an amendment was accepted saying that the surface must be conical. Thus, I don't have a very high opinion about discussions between lawyers on scientific questions, but this is not our business. I believe that space exploration after all is an analytical continuation, as the mathematician would say, of all we have done in the atmosphere.

I say, in space flight we even have similar economic questions as in air transportation. For example, if you calculate the cost per passenger mile for a journey to a far-away planet you will find that it is cheaper per passenger mile than to fly from Washington to New York. The cost consists essentially of the vehicle, amortization of ground equipment and actual expense of the launching; now even if these expenses amount to one or two million dollars, when you divide this amount by a distance of 400 million miles you get a cost per mile of a few cents. It is evident that per passenger mile space flight is comparable to current transportation by air. Of course, this is only a joke. However, I find that it is a serious thing for United States aeronautical engineers not to be discouraged. What we were learning, and what we were teaching, as well as the experiments we were making haven't lost their value because the frontiers of application have been extended. To be sure, one important change is necessary in education and also in the practice of the aeronautical engineer. We have to give much more attention to neighboring sciences, especially to the physical and chemical sciences. If you consider the development of aerodynamics you find that at the time we had low speeds and low altitudes a simple kind of aerodynamics did the trick. We assumed an approximation that the air is incompressible like water. We could then calculate lift and drag, especially after the concept of the boundary layer was brought in some 50 years ago, by Prandtl, so that we could also reasonably calculate friction forces. Then as the velocity increased, we came to the so-called sound barrier, and thermodynamics was necessary in addition to fluid mechanics. General Crocco, a pioneer of the Italian aviation, proposed the term "Aerothermodynamics" for this kind of science. Now, as we go to the highest altitudes and to the highest practical velocities, that is not sufficient. First, in highest altitudes -- as Dr. Kurzweg showed us today, we have the slip flow and the molecular flow -- I think it is unavoidable that an aeronautical engineer has to go back to statistical mechanics, to kinetic gas theory, and learn the facts of molecular motion. Then, because of the high temperatures we have dissociation, and eventually other chemical reactions. Now this marriage between aerodynamics and chemistry is also unavoidable for design of combustion chambers, or of gas turbines, jets and so on. I ventured a long word for this united science, calling it "Aerothermochemistry". It is a long word, but it seems there was a need for it because three months after I proposed the term four or five years ago, Northwestern University announced a symposium for "Aerothermochemistry".

Finally, if we go to space flight, it is electromagnetism that becomes important. After all, maybe we will have to resort to a new kind of propulsion like ionic propulsion or electromagnetic propulsion; and certainly we have to study things like the Van Allen radiation belt and in general radiation in space. Such phenomena also exist in the atmosphere. The new science combining fluid mechanics and electromagnetic theory needs a very urgent development. I think at the University of Maryland, New York University, and also at Cornell University, where I spent three months learning this new science from my own former students, they call it Magnetohydrodynamics. Now, I don't like that term. I would prefer using the expression Magnetofluidmechanics or Magnetofluiddynamics, but regardless of its name, there is no doubt that this new science has to be studied. Aerodynamics, I believe, and Aeroballistics, do not become obsolete. To the contrary, they become broader and more and more important -- maybe also more and more difficult. From this point of view we have to congratulate the United States Navy and its first-class facilities, and, what may be more important, first class personnel and consultants who are working in this direction. I think I express the feeling of all of you if we congratulate them and say, in Latin: "NOL, Vivat, Crescat, Floreat". Is it necessary to translate the words into English -- No? So I thank you.

DECENNIAL SYMPOSIUM

Tuesday, 26 May 1959

INTRODUCTORY REMARKS - YESTERDAY AND TOMORROW

by

Dr. R. J. Seeger
Acting Assistant Director
Mathematical, Physical and Engineering Sciences
National Science Foundation

It is always a joy to return "home" where one has lived and moved and had one's being even for a short while. I have always regarded it a rare privilege and a distinct honor to have had some share in the establishment of these Aeroballistic Research Facilities.

By way of introduction, let us note the origin of the term aeroballistics. "What's in a name!" You say impatiently. Certainly modern advertising has taught us the potential value of appropriate labels for promotional purposes. I recently attended an informal gathering of astronomers where everyone agreed that it would be academically desirable nowadays to emphasize modern dynamic astronomy rather than ancient celestial mechanics. My former colleaque, Dr. Kurzweg, once told me the following story as to the origin of the term hypersonic. In the late thirties some German aerodynamicists wished to procure a wind tunnel for air flow faster than the available speeds, which were already faster than sound, i.e., überschall (above sound). Accordingly, they made a Government application for a überschall tunnel (strangely enough, there being no German word for "much faster than sound.") A bureaucrat dutifully looked up the record and triumphantly declared that they already had a überschall tunnel. The embarrassed engineers went sorrowfully away. Some time later they returned with a request for a superschall tunnel (Latin above, German sound). This time the clerk could find no record of any such facility. Accordingly, the project was approved. All was well until the end of the war when the Americans decided to liberate these Kochel tunnels and transfer them to White Oak. How should the names be translated across the ocean? The German word überschall readily became the Latin-based English word supersonic (above sound). What about the word superschall? Some one had the bright idea to substitute the Greek equivalent hyper for the Latin prefix super, a linguistic hybrid -- hence the term hypersonic for wind tunnels with speeds much faster than sound. The novelty of these unique wind tunnels, as they were being redesigned and reconditioned in this country, attracted many visitors to the Naval Ordnance Laboratory. As a "brass" conductor I found myself being asked again and again by tourists why the Bureau of

Ordnance should be concerned with wind tunnels? NACA visitors were keenly sensitive of their cognizance over aeronautics; the Air Force was particularly jealous of its newly-acquired prerogatives; even the Navy Bureau of Aeronautics was admittedly perplexed at this seemingly competitive ordnance encroachment. A happy thought occurred to me -- to find a new descriptive name. Having recently come upon the apt term aerophysics for the borderline between aerodynamics and physics (cf. the dictionary), I invented the word aeroballistics (unfortunately another hybrid with Greek aero and Latin ballista). Higher-speed tunnels naturally led to the term hyperballistics. Believe it or not, from that time on visitors no longer questioned the activity; anything involving ballistics seemed to be obviously an ordnance program.

These terms, however, are not altogether as fanciful as they may appear from this historical derivation. Ballistics, as you know, started with fast projectiles in the form initially of rounded stones, then of elongated bodies, and finally of fin-stabilized missiles. Aeronautics, on the other hand, began with the comparatively low speeds of spheroidal balloons, which transformed later into winged aircraft and eventually into swept-back planes. At supersonic speeds ballistic and aeronautic bodies both merged into somewhat similar shapes; ballistics and aeronautics coalesced naturally in a single technical discipline. The term has become increasingly descriptive. I regret, however, that the word aeroballistics is not yet in the dictionary. Perhaps there is some comfort in the fact that the more glamorous astroballistics is also missing - not to mention rocketry, which has become a household word with children. Perhaps it would not be inappropriate for us all to encourage a wider usage of aeroballistics as an ordnance term.

Let us look for a moment at the meaning of the word "research", which also occurs in the name of these facilities. In this connection I am happy to be here as a representative of the National Science Foundation, which is vitally concerned with basic research in science. From the standpoint of development itself, one should continually be mindful that small-scale research is always much less costly than full-scale experiments, and certainly theory is cheapest of all. Of course, one must always ascertain carefully how theory is to be applied to any experiments and how small-scale experiments are to be related to the full-scale ones. In this land of apparent plenty we should never forget that there may always be more economic means of achieving the ends we have in view. Frankly, I have never been so much impressed by the possibility of new knowledge to be obtained directly by a trip to the moon; I am always thrilled more by the existence of old knowledge realized without any contact whatever, e.g., its size and distance. In this connection, I should like to quote from some remarks made at the 27 June 1949 general session of the Dedication of the NOL Aeroballistic Research Facilities:

> "Our plan of aeroballistic research will be based upon two presuppositions: (1) that experiential data are to be interpreted with respect to fundamental concepts and comprehensive theory, and (2) that theory, in turn, is not to be divorced from experimental confirmation."

This plan seems to me to be just ar sound today as when I first proposed it ten years ago.

Let us glance back at the 1949 symposia. There were technical sessions dealing with ordnance aeroballistics, with experimental compressible flow, and with theoretical compressible flow. Despite the higher speeds that have been subsequently achieved, the ordnance front is still in this region -- merely farther advanced. There were also scientific sessions dealing with aerothermodynamics, shockwave phenomena, and turbulence -- cosponsored by the American Physical Society Fluid Dynamics Division, the founding of which had been largely inspired by NOL physicists of the Aeroballistic Research Department. It is certainly unnecessary to point out to the group here that these basic research problems are still challenging us.

How has the world horizon changed since the 1949 symposia? In the first place, there is definitely a greater national interest in science, as seen in the establishment of the National Science Foundation itself in 1950. Secondly, international cooperation in science has expanded, as typified by the recently concluded International Geophysical Year. Finally, there has been an accelerated Soviet development in technology, as evident in Sputnick and Lunik, which show the Soviets having an outer look -- in addition, to their current open-door policy, which has given us an inner look. The recent announcement of their "Seven Year Plan" reveals an impressive future look. I would certainly be among those who would discount the claim of the Soviets to all the significant developments in modern aeronautics (cf. the 1 January 1949 issue of Pravda with its boasts that contemporary aerodynamics is due primarily to the pioneering work of Zhukovsky, D. I. Mendeleev, and Chaplygin.) On the other hand, we must admit that Western leadership is being challenged by a determined and dedicated group in the Soviet Union. It is not enough that we dedicate and rededicate these Aeroballistic Research Facilities. We must dedicate and rededicate ourselves to our unfinished tasks with them.

Let us briefly look ahead. What is the outlook for the next ten years? The history of fluid dynamics up to the twentieth century shows three major periods of development; hydrostatics, hydrodynamics, and aerodynamics. Our present age may properly be designated the physics of fluids, symbolized by the recent establishment of a journal along this line by the American Institute of Physics (edited by a former NOL colleague of mine.) By the physics of fluids, I mean the understanding of phenomena of ordinary fluids, of fluids under unusual physical conditions, and of fluids involving large-scale motions.

In the case of ordinary fluids, we have the perennial problems of stability, which is associated with the very foundations of fluid dynamics, and of viscosity, which is typified by the use of the term anomalous viscosity for normal phenomena (the anomaly consists chiefly in our inability to explain it). The various non-Newtonian substances, too, involve the very foundations of fluid dynamics. Recently a graduate student asked me about the flow of dirty water. As a theoretical physicist, I was aghast. We usually considered only TP substances, i.e., theoretically pure. At what grade level, I wondered, should one teach the properties of mud pies? Finally, we are still very ignorant of ever-present surface phenomena with their free boundaries, including non-linear waves, and cloud droplets.

Under the category of fluids with unusual physical conditions, we think at once of gases at high velocities with their compressible effects and interacting shockwaves. Gases at high altitudes exhibit low densities, where molecular physics has to be considered. For high temperatures, chemical reactions become significant, and for low temperatures quantum mechanics produces macroscopic effects in so-called superfluids. Here, too, we are evidently concerned with the fundamental properties of fluids.

Under the general heading of large-scale motions we note phenomena of a global character such as the flowing of silt-laden rivers and the blowing of weather-modifying winds. Atmospheric sciences of the earth include aurorae and air-glow, meteors and cosmic rays, et al. Nowadays we are particularly conscious of this region as we penetrate it with balloons and rockets and satellites. Leaving the planets, we are eager to explore the cosmic realm. Here fluids moving in magnetic fields experience electrodynamic forces, and electromagnetic effects, in turn, influence fluid motions. These interrelated phenomena are described by simultaneous non-linear equations both in magnetohydrodynamics dealing with continuous fluids, and in plasma physics involving ionized gases. Because of the great advantages now afforded by the development of high-speed calculating machines in the past ten years, I cannot refrain from calling attention to their potential usage in connection with all these research areas. Certainly the physics of fluids is fundamental to most ordnance problems and their solution will be impossible without an understanding of its foundations.

May I conclude with the same words that I used in 1949:

> "Our technical symposia this week symbolize, we believe, our research creed; (1) that NOL aeroballistic research must be a small, but creative part of today's aeromechanics research; (2) that naval ordnance scientific achievement will be possible only through cooperative partnership with people outside our own Laboratory."

HIGH TEMPERATURE GAS DYNAMICS

by

Dr. J. M. Burgers
Institute for Fluid Dynamics and Applied Mathematics
University of Maryland

Introduction

At the Dedication Ceremonies of the Aeroballistic Research Facilities of the U. S. Naval Ordnance Laboratory, White Oak, Maryland, in June 1949, I had been invited to give a talk on "Borderline Regions between Aerodynamics and Physics".1) In that talk I attempted to give a survey of several problems, taking as a guiding line the equations for one-dimensional flow with phenomena of shock waves, relaxation effects, condensation, heat flow and radiation. Much of what I brought forward looks somewhat primitive in retrospect. Knowledge has greatly advanced since then and it has appeared that the various interactions which can take place between the particles of a gas require deeper analysis, which in many respects is still beyond our means. I greatly appreciate that I have again been asked to speak at the present Dedication and Decennial Symposium. Since the time at my disposal is short, I thought that it might be worthwhile to discuss some aspects of particle interaction in gases 2) selecting three cases illustrating some questions which probably will require great attention in the near future. The three cases refer to: (a) interactions taking place at close binary encounters, (b) cooperative interactions of limited extent which must be considered when charged particles act upon each other with Coulomb forces, and (c) electric fields which appear when there are differences in the density of positive and negative charges extending over still larger distances.

In most practical problems of gas flow one will attempt to work with equations for the macroscopic flow, that is, with the continuum equations. Nevertheless it is often important to give attention to the particles composing the gas. This is necessary in particular when the gas consists of a mixture of various constituents, and when we are interested in changes of the composition with location and time, either as a result of diffusion, or of chemical reactions, or of still other forms of particle interaction. One then will have recourse to the Boltzmann equation, since the distribution of the translational velocities of the particles is of much importance, both for the behavior of the particles under exterior forces, and for the interactions which take place in close encounters. In a certain sense the Boltzmann equation describes the competition between the effects of exterior forces and the effects of mutual interactions. As regards the latter, what usually strikes us primarily is the randomizing of the translational velocities which is caused by the collisions, in consequence of which the distribution function is always driven towards the standard equilibrium form given by Maxwell. This standard form has isotrophy of the velocity distribution around a mean mass flow and leads to a scalar pressure. The randomizing effect of the collisions, however, is not always sufficient to achieve this result in the case of low density gases subjected to strong electric or magnetic fields. Also in the neighborhood of walls there may occur considerable deviations from the standard form if the density is low.

Hence a first classification of cases will distinguish between fields where the randomizing effect of collisions is preponderant, and fields where collisions are of minor importance. The distinction cannot always be made in a sharp way, since the effect of a collision is dependent upon the individual velocities of the particles.

A further distinction must be made between collisions in which both partners carry resultant electric charges, and all other cases. In the first case the interaction is determined by Coulomb forces, depending upon the product of the two resulting charges and decreasing inversely proportionally with the second power of the distance. This is a slow decrease, as is evident when one remembers that the number of particles at equal distance from a given particle increases directly proportionally with the same power of the distance. Hence we must expect that the interaction forces will have a long range, and the notion of simple collisions between two particles must be replaced by a more complicated form of interaction in which many particles will be involved.3)

When the particles do not both carry a resultant charge, electric forces still may play a part, but they will now depend upon dipoles or higher multipoles, while their full treatment must be based upon quantum theory, so that, if necessary, account may be taken of spin interaction. The resulting forces vary with higher inverse powers of the distance than the second power; hence their range is much more restricted and interaction occurs only at distances comparable to what is usually called the diameter of the particles. At such short distances the forces rapidly rise to very high values and the consequence is that the internal structure of the particles may change along with exchange of momentum and kinetic energy. Such changes can refer to transfer of energy towards internal motions (rotation, vibrations, higher electronic states, in general: transition of the system to another energy level), and this can go so far that ionization or other forms of dissociation may result, or that new combinations of atoms are formed. Thus reactions of various kinds are of great importance in all problems of gas flow at high temperatures, and it is necessary to take proper account of them in writing out the Boltzmann equation.

Example I. - A simplified form of the Boltzmann equation to take account of reactions between particles.

1. In the example to be considered we restrict to "close encounters", such as occur between particles which do not have both a resultant charge. While the main concern of collision theory in this case is with binary encounters, we must discuss whether other types may call for attention.

In the first place it is necessary to admit spontaneous reactions, which can occur in a particle without external stimulation. A spontaneous change of state of a single particle can take place when the particle is in a state of high energy and can get from there to a state of lower energy. The high energy state may be the result of a collision having taken place some time previously. The nature of the transition process and the rules to which it is subjected must be analysed on the basis of quantum theory, and it is from quantum theory that one must obtain data concerning the probability of the process. Instead of the probable frequency per particle for unit time, we can give its inverse, the relaxation time for the process.

The energy released in the process can appear in various forms, as radiation, as kinetic energy of masses emitted in the process (if the particle dissociates), or as kinetic (or internal) energy of another particle which happens to pass close by. From a formal point of view it may be convenient to treat the emission of radiation as a dissociation process leading to the emission of a photon, with attention to the energy and the momentum of the photon. - The spontaneous reaction in certain cases can be of a type in which no energy is given out, but there is a change of state to a level of equal energy with other degrees of freedom engaged (e.g., redistribution of energy over various types of oscillations).

In the theory of chemical reactions it is observed that certain reactions can occur only if three particles are available. When a single particle must be formed out of two, the conservation laws for momentum and energy can be satisfied together only when a third particle is available to take up certain amounts of energy and momentum. Such a process, however, can formally be treated as a rapid succession of two binary collisions, provided the combined particle formed in the first collision can temporarily store energy in some way, subjected to the chance that a spontaneous reaction may occur with dissociation of the combined particle into its original constituents. The process then would take place as follows: in a first collision the two partners unite,with storage of energy at some high quantum level; the high-energy particle thus formed can either dissociate, as indicated, in which case the net result would be the same as in an ordinary collision without reaction; or before dissociation occurs, it may meet a third particle, in such a way that part of its energy can be transferred to the latter, while the combined particle passes into a state of lower energy, which may be a stable state. If this happens the reaction has led to the formation of a stable compound. It can be shown that such a mechanism leads to the usual formula for the probability of a combination reaction in the presence of a third molecule.4)

Thus for a formal treatment we can restrict to the consideration of spontaneous reactions and binary collisions.

The probability of the occurrence of a binary collision is given by the product of the number densities of the two types of particles, of their relative velocity, and a collision cross section. Collision cross sections in some cases may be treated as constants, depending only upon the nature and the dimensions of the two colliding particles; but more usually they are quantities depending also upon the magnitude of the relative velocity. The collision may be followed either by a simple separation of the original particles without change in their internal states, or some reaction may take place. Hence a further probability factor is needed to describe the chance for each separate possibility. The sum of these factors over all possible occurrences (including simple separation) must be equal to unity.

The calculation of collision cross sections and probability factors belongs to the domain of quantum mechanics. Often the product of the collision cross section and the probability factor for a reaction is termed the "cross section for that particular reaction".

2. The treatment of flow problems by means of the kinetic picture needs distribution functions for the translational velocities of all the types of particles: molecules, atoms, radicals, electrons, etc. Properly speaking each quantum state must be considered as a separate type in this connection. However, for many purposes the notion of the distribution function can be extended in such a way that it also will give the distribution over the various possible quantum states. For convenience we will assume that the quantum states are labeled by a subscript $\underline{n}$, but it must be kept in mind that this

number in general will consist of several components, and that some components may have a continuous scale. 5)

For each type of particle the Boltzmann equation must describe the change of the distribution function as it results both from spontaneous processes and from binary collisions. Apart from the probability factors for these occurrences we need functions describing the probable distribution of translational velocities of the particles coming out either of a spontaneous reaction, or out of a binary collision with the possible concomitant reaction. In the classical standard case of a collision between two molecules with spherical symmetry, where it is assumed that no reaction occurs nor energy exchange with internal degrees of freedom, the velocities of the partners after the collision can be expressed exactly in terms of their velcities before the collision and two parameters specifying the geometrical aspects of their approach. The velocity of the center of mass of the system remains unchanged both in magnitude and in direction; the relative velocity is not changed in absolute magnitude, and a probability law can be derived for the angle which the final vector makes with the initial direction, provided that the force law is known. 6) The position in space of the plane containing these two vectors has a uniform distribution over all angles around the initial vector.

The problem becomes much more complicated in the case where there is exchange of energy with internal degrees of freedom (this includes rotational energy), or when new particles are formed. The proper treatment is a matter for quantum theory and the consideration of a large number of parameters may be necessary before the desired results can be obtained. In most cases the problems arising here are so formidable that it is necessary to look for possible simplifications and approximations. In view of this circumstance it seems appropriate to treat also ordinary binary collisions (without reactions) in an approximate way. Molecules in general are not spherically symmetric, nor are the force laws always of the types as have been considered in the literature. Consequently a procedure indicated by Max Krook for obtaining a linearized form of the Boltzmann equation becomes of great use, since it can be extended to the case of reactions. 7)

In Krook's approximation it is assumed that the distribution of the translational velocities of the resulting particles can be represented by a Maxwellian function, in which mean flow velocity and temperature must be adjusted properly. Instead of having to consider a completely unknown distribution function, it thus is assumed that for each type of particle there are only three characteristic parameters: the number of particles coming out of the process, their mean flow velocity, and their mean temperature. In general these quantities will be different for the various products of the process. Certain relations between the parameters follow from the conservation laws which must be satisfied. In general, however, the number of adjustable parameters will exceed the number of equations; supplementary conditions must be found from a quantum theoretical treatment. Where this is not available - and so far this represents the majority of cases - guesses must be made, based upon a discussion of the nature of the process.

The idea can be extended by assuming that also the distribution over the various quantum levels is governed by an exponential function depending upon the energy. In such a case it may be necessary to suppose that the temperature introduced into the distribution over the levels of rotational energy, and the temperature introduced into the distribution over the levels of vibrational energy, and temperatures introduced into the distribution over other sets of energy levels, can be different from each other and also different from the temperature used in the function referring to the translational energy.

It seems probable that the mean flow velocities of all particles coming out of a single type of process will have the same value; this value can then be found from the mean flow velocities of the particles involved before the process occurred, since momentum must be conserved.

With respect to temperature a similar supposition is not always permitted. For instance in collisions between atoms and electrons, exchange of translational energy is so ineffective that it is better to assume that the electrons keep the same temperature as they had before the collision. 8) Similarly, exchange between translational and internal energy may be slow. What to assume from the mean translational temperatures of the particles produced by dissociation is still in doubt, in particular when there is a great difference of mass, as between electrons and ions produced in an ionization process.

3. We now will give an example of the general form which the Boltzmann equation obtains, when account is taken of spontaneous processes and of reactions resulting from binary collisions. The following notations are used:

$F_{a,n}$ is the distribution function for the velocity components ξ_{ah} of the particles of type $\underline{a}$, in the quantum state $\underline{n}$ (the second subscript, h, added to the velocity component, refers to the coordinate directions: h = 1, 2, 3; summation over the coordinate directions is not written explicitly). The mass of a particle of type $\underline{a}$ is written m_a. The f_{ah} are the components of the exterior forces acting on these particles; in most cases they can be assumed to be independent of the quantum state $\underline{n}$.

General form of Boltzmann equation:

$$\frac{\partial F_{a,n}}{\partial t} + \xi_{ah} \frac{\partial F_{a,n}}{\partial x_{ah}} + \frac{f_{ah}}{m_a} \frac{\partial F_{a,n}}{\partial \xi_{ah}} =$$

$$= - F_{a,n} \left[\sum_i \frac{1}{\tau_i} + \sum_{b,n'} N_{b,n'}\, g_{ab}\, S_{a,n;b,n'} \right] +$$

$$+ \sum_{b,n'/j} \frac{N_{b,n'}}{\tau_j} \nu_{j/a} \Phi_{j/a,n} +$$

$$+ 1/2 \sum_{b,n';c,n''/\ell} N_{b,n'} N_{c,n''}\, g_{bc}\, S_{b,n';c,n''}\, P_{b,n';c,n''/\ell}\, \psi_{\ell/a}\, \Phi_{\ell/a,n} \ .$$

In the first line on the right hand side of the equation the summation with respect to i refers to the various possible spontaneous processes to which a particle of type a, in quantum state n, may be subjected; the relaxation times for these processes are written τ_i.

The other term in this line, starting with the summation with respect to b and n', refers to collisions of particles a in quantum state n with particles of type b in a quantum state n'. Here g_{ab} is the absolute value of the relative velocity of the particles; $S_{a,n;b,n'}$ is the relevant cross section, which may depend upon the magnitude of g_{ab}. In many approximations a mean value is used for g_{ab}, calculated from the temperature and the masses of the colliding particles. - The summation with respect to b and n' must include also particles of type a and the quantum state n.

Together the two terms of this line represent the chance that a particle a,n is removed from the set to which $F_{a,n}$ refers, either in consequence of a spontaneous reaction or of a collision.

The other two lines on the right hand side give the chance that particles a in quantum state n may come out of spontaneous reactions or out of collisions. Such particles may come from particles b in state n' if there is a spontaneous reaction j producing $\nu_{j/a}$ particles a from a single particle b . The function $\Phi_{j/a,n}$ is the distribution function assumed for the distribution of these particles over the quantum states n and for the translational velocities. In general this function will be of the form:

$$\Phi_{j/a,n} = \varphi_{j/a,n} \left(\frac{m_a}{2\pi k T_{j/a}} \right)^{3/2} \exp \frac{-m_a (\underset{\sim}{\xi}_a \quad \underset{\sim}{u}_j)^2}{2 k T_{j/a}}$$

Here the u_{jh} are the components of the flow velocity of the particles coming out of the spontaneous process, assumed to be independent of the type and the quantum state of these particles, while the $T_{j/a}$ are the temperatures. The function $\varphi_{j/a,n}$ must give the distribution over the quantum states n ; sometimes the following form may be assumed for this function:

$$\varphi_{j/a,n} = \alpha_{a,n} \exp (- \varepsilon_{a,n}/kT_{j/a}) \quad ,$$

where the $\varepsilon_{a,n}$ are the energies connected with the levels n , while the factors $\alpha_{a,n}$ are connected with the multiplicity of the levels. It may be, however, that more complicated formulas are desirable in cases where various degrees of freedom must be considered. - In all cases the function $\varphi_{j/a,n}$ is normalized in such a way that summation over all quantum states n will give unity.

Similar observations hold for the last line of the equation, giving the number of particles a in quantum state n coming out of collisions between particles b,n' and particles c,n". The $S_{b,n':c,n''}$ again are cross sections for the collisions; the factors $P_{b,n';c,n''/\ell}$ are the probability factors for the various possible reactions ℓ . The summations with respect to b,n' and with respect to c,n" also include a,n ; and the factor 1/2 has been introduced since complete summation counts every collision twice.

The formalism can be extended to such cases where a photon occurs, either as a product of a reaction or as a collision partner.

The Boltzmann equation can be used as a basis for the deduction of equations for the conservation of mass, of momentum and of energy. I will leave, however, this subject here; it is hoped that various further details and applications may be treated in a separate report.

Example II. - The treatment of collisions between charged particles with the aid of a two-particle distribution function.

4. The present example will refer to cooperative interactions of limited extent appearing when charged particles act upon each other with Coulomb forces. Such interactions occur in a completely ionized gas of plasma. In recent years several papers have been published on the treatment of these interactions with the aid of so-called "two-particle distribution functions", as introduced by Born and Green, Kirkwood, and Bogoliubov. 9) Of particular interest in this connection has been a paper by S. W. Temko 10) and its analysis and re-formulation by C. M. Tohen, who has made the treatment more systematic and who has introduced several improvements and extensions 11).

Both Temko's and Tohen's methods of treatment of the problem lead to final equations of the Fokker-Planck type. The same holds for a treatment of the problem by Gasiorowicz, Neumann and Riddell 12), who start, however, from a different point of view. An important feature in the deductions of all these authors is the application of Fourier (or Laplace) transforms for the coordinate dependence of the distribution functions. Which such a treatment distinguishes itself through its elegance, it has the disadvantage that it requires a form of approximation which obscures certain questions connected with the occurrence of close encounters and introduces a divergence in certain integrals.13) I have therefore considered whether it would be possible to change the treatment in such a way that it can be fully described in coordinate language without the introduction of Fourier transforms. The result arrived at is presented in the following pages; it obviates the use of these divergent integrals.

In view of the difficult nature of the problem all complications connected with reactions as considered in the previous example will be left aside.

All particles are assumed to have spherical symmetry and to occur only in a single state, so that energy levels and transitions between them do not occur. Nor are there spontaneous reactions or energy exchange with radiation. Also exterior forces will be left out of the picture. All attention is concentrated upon the interactions between the particles. Since these are of Coulomb type, the interaction between any two particles with charges e_a, e_b can be described by a Coulomb potential function

$$\Phi_{ab} = e_a \, e_b \, / \, r_{ab} \quad , \tag{1}$$

where r_{ab} is the distance between the particles at the instant considered.

A somewhat different definition will be used with regard to the distribution functions for the translational velocities of the various types of particles, in so far as these functions will be normalized according to the equation:

$$\int F_a \, d\underset{\sim}{\xi}_a = 1 \, , \tag{2}$$

Thus the number density N_a of the particles $\underline{a}$ per unit volume does not occur in F_a, but must be introduced separately where necessary.

The two-particle distribution function F_{ab} refers to the probability 14) that at the instant t we shall find a particle $\underline{a}$ within a definite element $d\underset{\sim}{x}_a$ of ordinary space, its velocity vector reaching into a definite element $d\underset{\sim}{\xi}_a$ of the velocity space, while simultaneously there is a particle $\underline{b}$ in $d\underset{\sim}{x}_b$ with velocity vector in $d\underset{\sim}{\xi}_b$. This function is normalized in such a way that:

$$\int d\underset{\sim}{x}_b \int d\underset{\sim}{\xi}_a \int d\underset{\sim}{\xi}_b \; F_{ab} = V \, , \tag{3}$$

where V is the volume of the coordinate space over which the integration is carried out. This volume must be large compared with the distances over which there is an appreciable interaction between the particles, 15) but at the same time it must be small in comparison with distances over which there is a marked change in the field. This means that the state of the field at any instant can be treated as if it is uniform over a volume V. The three-particle distribution function F_{abc} is normalized in a similar way, but with more integrations. - We shall not introduce functions referring to more than three particles.

5. The functions F_a and F_{ab} are subjected to the following equations:

$$(4)\quad \frac{\partial F_a}{\partial t} + \xi_{ah}\frac{\partial F_a}{\partial x_{ah}} = \sum_b N_b \iint d\underset{\sim}{x_b}\, d\underset{\sim}{\xi_b}\, \frac{1}{m_a}\frac{\partial \Phi_{ab}}{\partial x_{ah}}\frac{\partial F_{ab}}{\partial \xi_{ah}};$$

$$(5)\quad \begin{cases} \dfrac{\partial F_{ab}}{\partial t} + \xi_{ah}\dfrac{\partial F_{ab}}{\partial x_{ah}} + \xi_{bh}\dfrac{\partial F_{ab}}{\partial x_{bh}} - \dfrac{1}{m_a}\dfrac{\partial \Phi_{ab}}{\partial x_{ah}}\dfrac{\partial F_{ab}}{\partial \xi_{ah}} - \dfrac{1}{m_b}\dfrac{\partial \Phi_{ab}}{\partial x_{bh}}\dfrac{\partial F_{ab}}{\partial \xi_{bh}} = \\ = \displaystyle\sum_c N_c \iint d\underset{\sim}{x_c}\, d\underset{\sim}{\xi_c}\left[\frac{1}{m_a}\frac{\partial \Phi_{ac}}{\partial x_{ah}}\frac{\partial F_{abc}}{\partial \xi_{ah}} + \frac{1}{m_b}\frac{\partial \Phi_{bc}}{\partial x_{bh}}\frac{\partial F_{abc}}{\partial \xi_{bh}}\right]. \end{cases}$$

We shall write:

$$(6)\quad F_{ab} = F_a\,F_b + F_{ab}\,' :$$

$$(7)\quad F_{abc} = F_a F_b F_c + F_a F_{bc}\,' + F_b F_{ac}\,' + F_c F_{ab}\,' + F_{abc}\,' .$$

These notations do not involve a loss of generality so long as nothing is specified about the functions $F_{ab}\,'$ and $F_{abc}\,'$. When these expressions are substitutes into eqs. (4) and (5) some terms drop out in consequence of eq. (2) in connection with the circumstance that the potentials Φ_{ab} depend upon differences of coordinates. Equation (4) is changed in such a way that on the right hand side one obtains $F_{ab}\,'$ in the place of F_{ab}. Equation (5), after some re-arrangements, can be brought into the form

$$
(8)\quad \begin{cases}
\dfrac{\partial F_{ab}'}{\partial t} + \xi_{ah}\dfrac{\partial F_{ab}'}{\partial x_{ah}} + \xi_{bh}\dfrac{\partial F_{ab}'}{\partial x_{bh}} - \\[2ex]
-\dfrac{1}{m_a}\left[\dfrac{\partial \Phi_{ab}}{\partial x_{ah}}\dfrac{\partial F_{ab}'}{\partial \xi_{ah}} + \sum_c{}' N_c \iint d\underset{\sim}{x}_c\, d\underset{\sim}{\xi}_c \dfrac{\partial \Phi_{ac}}{\partial x_{ah}}\dfrac{\partial F_{abc}'}{\partial \xi_{ah}} - \right. \\[2ex]
\left. -\dfrac{1}{m_b}\left[\dfrac{\partial \Phi_{ab}}{\partial x_{bh}}\dfrac{\partial F_{ab}'}{\partial \xi_{bh}} + \sum_c{}' N_c \iint d\underset{\sim}{x}_c\, d\underset{\sim}{\xi}_c \dfrac{\partial \Phi_{bc}}{\partial x_{bh}}\dfrac{\partial F_{abc}'}{\partial \xi_{bh}}\right] = \right. \\[2ex]
= \dfrac{1}{m_a}\dfrac{\partial F_a}{\partial \xi_{ah}}\left[\dfrac{\partial \Phi_{ab}}{\partial x_{ah}} F_b + \sum_c{}' N_c \iint d\underset{\sim}{x}_c\, \underset{\sim}{\xi}_c \dfrac{\partial \Phi_{ac}}{\partial x_{ah}} F_{bc}'\right] + \\[2ex]
+ \dfrac{1}{m_b}\dfrac{\partial F_b}{\partial \xi_{bh}}\left[\dfrac{\partial \Phi_{ab}}{\partial x_{bh}} F_a + \sum_c{}' N_c \iint d\underset{\sim}{x}_c\, d\underset{\sim}{\xi}_c \dfrac{\partial \Phi_{bc}}{\partial x_{bh}} F_{ac}'\right]
\end{cases}
$$

So far no approximations have been introduced.

6. In order to be able to make use of these equations we must introduce an assumption concerning the function F_{abc}', of such nature that this function will be related to functions of type F_a or F_{ab}. We observe that F_{abc}' occurs only under an integration, so that there can be some hope that the inevitable inaccuracy of any assumption will be softened in the result. At the same time we observe that there are integrals in which occur the functions F_{ac}' and F_{bc}'. We shall also assume that in these integrals - but not elsewhere - these functions may be replaced by approximations, and we will choose the expression to replace F_{abc}' in close relation with the approximation used for F_{ac}' and F_{bc}'.

The particular assumptions which we shall introduce are as follows: 16)

$$(9) \qquad F_{ac}' = -F_a F_c \frac{\Psi_{ac}}{kT_o}; \quad F_{bc}' = -F_b F_c \frac{\Psi_{bc}}{kT_o} \quad ;$$

$$(10) \qquad F_{abc}' = -F_{ab}' F_c \frac{\Psi_{ac} + \Psi_{bc}}{kT_o}$$

(in the latter formula <u>no</u> approximation is introduced for F_{ab}'). Here Ψ_{ac} and Ψ_{bc} are functions of the distances r_{ac} and r_{bc}, respectively (symmetric in the two subscripts), of the nature of a potential energy, that is, having the dimensions of $e_a e_c / r_{ac}$ or $e_b e_c / r_{bc}$, so that their quotients by kT_o are dimensionless. T_o is a suitably chosen mean temperature.

We attempt to determine the functions Ψ in such a way that the following equation will hold:

$$(11) \qquad \Psi_{ab} = \Phi_{ab} - \sum_c N_c \int d\underset{\sim}{x}_c \frac{\Phi_{ac} \Psi_{bc}}{kT_o} .$$

If we write $\Psi_{ab} = e_a e_b \psi_{ab}$, this equation becomes:

$$\psi_{ab} = \frac{1}{r_{ab}} - \sum_c \frac{N_c e_c^2}{kT_o} \int d\underset{\sim}{x}_c \frac{\psi_{bc}}{r_{ac}} .$$

Application of the Laplace operator Δ with respect to the coordinates x_{ah} transforms this into:

$$(12) \qquad \Delta \psi_{ab} = - \sum_c \frac{N_c e_c^2}{kT_o} \int d\underset{\sim}{x}_c \, \psi_{bc} \Delta \left(\frac{1}{r_{ac}} \right) = \varkappa^2 \psi_{ab} ,$$

where

$$\varkappa^2 = \sum_c \frac{4\pi N_c e_c^2}{k T_o} \qquad (13)$$

It will be recognized that the latter formula characterizes the parameter occurring in the theory of the Debye screening effect.

It is found that the approximate solution of eq. (12) is:

$$\psi_{ab} = \frac{\exp(-\chi r_{ab})}{r_{ab}} , \qquad (14)$$

and it can be verified that this solution satisfies eq. (11).

When use is made of this result, equation (8) for F_{ab}' is transformed into:

$$(15) \left\{ \begin{aligned} &\frac{\partial F_{ab}'}{\partial t} + \xi_{ah} \frac{\partial F_{ab}'}{\partial x_{ah}} + \xi_{bh} \frac{\partial F_{ab}'}{\partial x_{bh}} - \\ &\qquad - \frac{1}{m_a} \frac{\partial \Psi_{ab}}{\partial x_{ah}} \frac{\partial F_{ab}'}{\partial \xi_{ah}} - \frac{1}{m_b} \frac{\partial \Psi_{ab}}{\partial x_{bh}} \frac{\partial F_{ab}'}{\partial \xi_{bh}} = \\ &= - \left(\frac{F_b}{m_a} \frac{\partial F_a}{\partial \xi_{ah}} - \frac{F_a}{m_b} \frac{\partial F_b}{\partial \xi_{bh}} \right) \frac{\partial \Psi_{ab}}{\partial x_{ah}} . \end{aligned} \right.$$

7. Equation (15) is a linear partial differential equation linking the function F_{ab}' with the functions F_a and F_b. If the latter are supposed to be given, the equation can be integrated along its characteristics. These characteristics describe the motion of the two particles a and b , under the influence of a mutual interaction described by the potential function Ψ_{ab} ; Since this function is of a more complicated nature than the Coulomb potential, there does not exist a simple description of the orbits as can be deduced from the theory of the Kepler motion; nevertheless, in principal it is possible to construct the orbits, starting from a situation where a particle a is at the point x_{ah} with the velocity ξ_{ah} , and a particle b at the point x_b with the velocity ξ_{bh} , at the instant t . We construct these orbits backwards in the time direction and assume that we can take $F_{ab}' = 0$ for $t = -\infty$ 17). In this way a fully determinate falue of F_{ab}' can be obtained.

We observe that the orbits will approximate to straight lines as soon as the distance between the particles has increased to a few times the Debye length, since the potential function Ψ_{ab} then will become very small. If we assume that the relative velocity of the particles has a mean value connected with the mean temperature of the gas, we can also introduct the notion of a certain mean interaction time. Our starting equations and the normalizing condition (3) for F_{ab} involved the assumption that the mean field was nearly uniform over distances of the order of the Debye length. We can supplement this by the assumption that the mean field does not change very much in a time interval of the order of the mean interaction time defined above. Under these circumstances we may consider F_a and F_b in first approximation to be constant when performing the integration along the characteristics. Slow changes of F_a and F_b with the coordinates or with the time can be taken into account, if desired, by making use of a development into a Taylor series. 18)

In principle an expression for F_{ab}' which would be obtained by integration along the characteristics with unspecified forms of F_a and F_b, can be substituted into eq. (4), which then turns into a non-linear equation for the distribution functions F_a and F_b , somewhat comparable with the Boltzmann equation which has a similar non-linear expression on the right hand side. 19)

Another method is to linearize the system of equations for F_a and F_b on the right hand side of (15), and express F_{ab}' as a linear function of

F_a and $\partial F_a / \partial \xi_{ah}$. The result could be substituted again into eq. (4), which then turns into an equation for F_a of the Fokker-Planck type.

Any procedure to be carried out on these lines will need the introduction of further approximations, since the direct integration of (15) alongs its characteristics is not a simple matter. It may be possible to divide the space around a particle into two parts, where different types of approximation can be used. It is hoped that we may come back to this subject in a later publication.

Example III. - Plasma Oscillations.

8. In the preceding example no particular attention was given to the number densities of the various kinds of particles. In studying the cooperative effects extending over a few times the Debye distance it is customary to assume that the average densities of positively and negatively charged particles are related in such a way that the gas contained within a volume of the order V is neutral.

This will no longer be the case when inertia effects come into play and when the particles have greatly different masses. In such a case charge separation appears in the field. Although it may be small when compared to the total number densities of the positive and negative ions, it is sufficient to produce an electric field. This field must be introduced into the Boltzmann equation as an "exterior field", since it arises from the behavior of the system at large distances from the particles under consideration. The relation between this field and the non-uniformity of the charge density is given by Poisson's equation.

We will consider the effect of this field for the case of oscillations in the direction of the x_1-axis, where the state of the field is assumed to be a periodic or damped periodic function of x_1 and t . The electric field then has a component E in the x_1-direction. It is supposed that there is no magnetic field. 20)

The equation to be used will be the ordinary Boltzmann equation with Krook's approximation for the collision term, as explained in example I. Reactions ; internal states will be left out of consideration; neither will interaction with a radiation field be considered. Only two kinds of ions are assumed; we use the subscript 1 for the positive ions, and 2 for the negative ions (electrons). The masses of the particles are m_1 , m_2 , respectively; the charges $e_1 = e$; $e_2 = -e$.

The equation for the distribution functions F_a $(a = 1, 2)$ will be written in the form:

$$\frac{\partial F_a}{\partial t} + \xi_{a1} \frac{\partial F_a}{\partial x_1} + \frac{e_a E}{m_a} \frac{\partial F_a}{\partial \xi_{a1}} =$$

(1)

$$= -(\omega_{a1} + \omega_{a2}) F_a + N_{a1} \omega_{a1} \Phi_{a1} + N_{a2} \omega_{a2} \Phi_{a2} .$$

The following notation is used:

ω_{ab} is the collision frequence for a single particle a with particles b, in unit time. Since the relative difference of the number densities of the positive and negative particles will be small, we can assume $\omega_{12} = \omega_{21}$. We also write:

$$\omega_{ao} = \omega_{a1} + \omega_{a2} .$$

N_{ab} is the number of particles a coming out of collisions with particles b, in unit time. We write:

$$N_{ab} = N_o (1 + n_{ab}),$$

where N_o is the equilibrium density of both types of ions. The numbers n_{ab} are supposed to be small compared with unity. Φ_{ab} is the distribution function for the velocities of the particles a coming out of a collision with particles b. These functions are given by:

$$(2) \quad \Phi_{ab} = \left(\frac{m_a}{2 \pi k T_{ab}}\right)^{3/2} \exp \frac{-m_a (\xi_a - u_{ab})^2}{2 k T_{ab}} ;$$

where u_{ab} is the mean flow velocity of these particles (parallel to the x_1-axis) and T_{ab} their mean temperature. We write:

$$T_{ab} = T_o (1 + \vartheta_{ab}),$$

where T_o is the equilibrium temperature of the gas, while the ab are small compared with unity. If we introduce:

$$(3) \qquad \Phi_a = \left(\frac{m_a}{2\pi k T_o}\right)^{3/2} \exp \frac{-m_a \underset{\sim}{\xi}_a^2}{2kT_o} \quad ;$$

$$(4) \qquad c_a^2 = 2kT_o / m_a \quad :$$

we can write:

$$(5) \qquad \Phi_{ab} = \Phi_a \left\{ 1 + \frac{2\xi_{a1} \quad u_{ab}}{c_a^2} + \left(\frac{\xi_a^2}{c_a^2} - \frac{3}{2}\right) \vartheta_{ab} \right\}.$$

It is assumed that all u_{ab} are small with respect to c_1 and c_2 .

In order to express that the distribution functions F_a do not greatly differ from their equilibrium forms, we write:

$$(6) \qquad F_a = N_o \Phi_a + F_a' \quad .$$

We further introduce quantities n_a , u_a , ϑ_a by means of the formulas:

$$(7) \qquad \int F_a' \, d\underset{\sim}{\xi}_a = N_o \, n_a \quad ;$$

$$(8) \qquad \int F_a' \, \xi_{a1} \, d\underset{\sim}{\xi}_a = N_o \, u_a \quad ;$$

$$(9) \qquad \int F_a' \, \xi_a^2 \, d\underset{\sim}{\xi}_a = \frac{3}{2} N_o \, (n_a + \vartheta_a) \, c_a^2 \quad .$$

It was supposed that $u_a \ll c_a$; on this basis a term with u_a^2 has been neglected in form. (9)

We finally need an equation linking E with the number densities:

$$(10) \qquad \partial E/\partial x_1 \quad 4\pi N_o e \; (n_1 - n_2) \quad .$$

Since there is no magnetic field, E can be written as the derivative of a potential:

$$E \quad -\partial\varphi/\partial x_1 \quad .$$

9. The quantities n_{ab}, u_{ab} and ϑ_{ab} are unknowns of the problem. For their determination we have the following relations:

(a) conditions referring to collisions between like particles:

$$(11) \qquad u_{aa} = u_a \; ; \quad \vartheta_{aa} = \vartheta_a \qquad (\text{for } a = 1,2) \quad ;$$

(b) conservation conditions : the numbers of particles a and particles b are both conserved ; the total momentum of the particles a and b together is conserved ; the total energy is conserved.

The conservation conditions for the numbers of particles give the two relations :

$$(12) \qquad \omega_{ao} n_a = \omega_{a1} n_{a1} + \omega_{a2} n_{a2} \quad (a = 1, 2) \; .$$

The conservation condition for the total momentum gives :

$$m_1 u_1 (\omega_{11} + \omega_{12}) + m_2 u_2 (\omega_{21} + \omega_{22}) =$$

$$= m_1 (\omega_{11} u_{11} + \omega_{12} u_{12}) + m_2 (\omega_{21} u_{21} + \omega_{22} u_{22}) \quad .$$

As mentioned in the treatment of example I (near the end of section 2) it is probable that we may assume $u_{12} = u_{21}$. We then find :

$$(13) \qquad u_{12} = u_{21} = \frac{m_1 u_1 + m_2 u_2}{m_1 + m_2} \quad .$$

The conservation condition for the total energy gives:

$$(\omega_{11}+\omega_{12})\vartheta_1+(\omega_{21}+\omega_{22})\vartheta_2 =$$
$$=\omega_{11}\vartheta_{11}+\omega_{12}\vartheta_{12}+\omega_{21}\vartheta_{21}+\omega_{22}\vartheta_{22} ,$$

from which:

$$\vartheta_{12}+\vartheta_{21}=\vartheta_1+\vartheta_2 . \tag{14}$$

For particles of nearly the same mass we can probably assume

$$\vartheta_{12}\cong\vartheta_{21}=1/2(\vartheta_1+\vartheta_2) . \tag{15a}$$

Such an assumption will not be applicable to collisions between electrons and heavy ions, where the transfer of energy in a single encounter is very small. A better assumption for this case will be :

$$\vartheta_{12}=\vartheta_1 ; \quad \vartheta_{21}=\vartheta_2 . \tag{15b}$$

10. We now must find equations for the quantities n_a, u_a, ϑ_a. For this purpose we must obtain a solution of the Boltzmann equation. When the expressions (5) and (6) are substituted into eq. (1) we can linearize still further in view of the circumstance that also E is a small quantity. The following result is obtained :

$$(16)\left\{\begin{aligned}&\frac{\partial F_a'}{\partial t}+\xi_{al}\frac{\partial F_a'}{\partial x_l}+\omega_{ao}F_a' =\\ &=\frac{N_o e_a E}{k T_o}\xi_{al}\Phi_a+N_o\Phi_a\left\{\alpha_a+\frac{2\xi_{al}}{c_a^2}\beta_a+\left(\frac{\xi_a^2}{c_a^2}-\frac{3}{2}\right)\gamma_a\right\},\end{aligned}\right.$$

where we have written:

(17a) $$\alpha_a = \omega_{ao}\, n_a \qquad ;$$

(17b) $$\beta_a = \omega_{ao}\, u_a - \frac{\omega_{ab}\, m_b}{m_a + m_b}(u_a - u_b\ ;$$

(17c) $$\gamma_a = \omega_{ao}\, \vartheta_a - \omega_{ab}(\vartheta_a - \vartheta_{ab}) ,$$

with $b \neq a$ in (17b) and (17c) .

We consider the propagation of progressive waves through the gas and assume that all variable quantities are proportional to the factor

$$\exp\left[i\ (\nu x_1 + \omega t) \right] .$$

We then have: $E = - i \nu \varphi$, and Poisson's equation (10) gives :

(18) $$\varphi = \frac{4\pi N_o\ e}{\nu^2}(n_1 - n_2) .$$

It is convenient to introduce the dimensionless quantity :

(19) $$\mathcal{E} = \frac{e\varphi}{k\,T_o} = \frac{4\pi N_o\ e^2}{k\,T_o\ \nu^2}(n_1 - n_2) .$$

In consequence of the exponential dependence of all variable quantities on x_1 and t the partial differential equation (16) transforms into an ordinary linear equation. The following solution is obtained:

(20) $$F_a{}' = \frac{-N_o\ \Phi_a}{\nu \xi_{a_1} + \omega_a^*}\left[\frac{e_a}{e}\mathcal{E}\nu\xi_{a1} + i\alpha_a + i\frac{2\xi_{a1}\beta_a}{C_a^2} + i\left(\frac{\xi_a^2}{C_a^2} - \frac{3}{2}\right)\gamma_a\right],$$

where $\omega_a^* = \omega - i\,\omega_{ao}$

To find n_a, u_a, ϑ_a we must carry out the integrations indicated in formulas (7), (8), (9). In view of the denominator occurring in (20) the integrals involve the transcental function

$$Q_a = \frac{1}{\sqrt{\pi}} \int_{-\infty}^{+\infty} dz \, \frac{\exp(-z^2)}{z + \varpi_a^*} \tag{21}$$

where the following nondimensional quantities are used:

$$\varpi_a = \frac{\omega}{\nu c_a}\,; \quad \varpi_{ab} = \frac{\omega_{ab}}{\nu c_a}\,; \quad \varpi_{ao} = \frac{\omega_{ao}}{\nu c_a}\,; \quad \varpi_a^* = \varpi_a - i\,\varpi_{ao}. \tag{22}$$

It should be noted that $\varpi_{ab} \neq \varpi_{ba}$. We have:

$$\frac{\varpi_a}{\varpi_b} = \frac{\varpi_{ab}}{\varpi_{ba}} = \frac{c_b}{c_a} = \left(\frac{m_a}{m_b}\right)^{1/2} \tag{22a}$$

The integrations lead to rather complicated equations. They can be brought into a simpler form (without loss of terms) by suitably combining them. The results can then be written in the following way :

$$u_a = -(\omega/\nu)\,n_a\,, \text{ or } \quad u_a / c_a = -\varpi_a\,n_a\,; \tag{23}$$

$$\left\{\begin{aligned} &(2\varpi_a^2 - 1)\,n_a - (3 + 2i\,\varpi_{ao}\,Q_a)\,\vartheta_a + 2i\,\varpi_{ab}\,Q_a(\vartheta_a - \vartheta_{ab}) = \\ &= \left\{ \frac{4\pi N_o c^2}{k\,T_o\,\nu^2} + 2i\,\varpi_a\,\varpi_{ab}\,\frac{m_b}{m_a + m_b} \right\}(n_a - n_b) \end{aligned}\right. \tag{24}$$

$$(25)\begin{cases} \tilde{\omega}_a \; (2\tilde{\omega}_a^* + Q_a - 2\tilde{\omega}_a^{*2} \; Q_a) \; n_a = \\ = 3(1 - \tilde{\omega}_a^* \; Q_a) \vartheta_a + i \; (\tilde{\omega}_a^* + \frac{5}{2} \; Q_a = \tilde{\omega}_a^{*2} \; Q_a - 2 \; \tilde{\omega}_a^* \; Q_a^2) \; \cdot \\ \cdot \left[\tilde{\omega}_{ao} \vartheta_a - \tilde{\omega}_{ab} (\vartheta_a - \vartheta_{ab}) \right] \; . \end{cases}$$

In order to obtain eq. (24) use has been made of the relation (19). In consequence of this eq. (24) can be applied to obtain the frequencies of free oscillations with a given value of (dispersion relation).

11. When eq. (24) is multiplied through with $k T_o \quad ^2$, it can be written in the form :

$$(24a)\begin{cases} \left[\left[m_a \omega^2 - kT_o \nu^2 \left\{ 1 + (3 + 2i \tilde{\omega}_{ao} Q_a) \frac{\vartheta_a}{n_a} - 2i \tilde{\omega}_{ab} Q_a \frac{\vartheta_a - \vartheta_{ab}}{n_a} \right\} \right] n_a = \right. \\ = (4\pi N_o e^2 + i \;\; \omega \omega_{12} \mu) \; (n_a - n_b) \; . \end{cases}$$

Here $\mu = m_1 m_2/(m_1 + m_2)$; also $\omega_{ab} = \omega_{ba}$ has been written as ω_{12}.

It can be expected from a preliminary discussion of eq. (25) that the ratios ϑ_a/n_a and $(\vartheta_a = \vartheta_{ab})/n_a$ will never be large compared with unity. This makes it possible to discuss two limiting cases of eq. (24a) in a simple way.

(A) We assume that $m_a \omega^2 \gg kT_o \nu^2$ for $a = 1, 2$ (in an ionized gas we require that the relation holds with m_2 = electron mass). The first term between the square brackets on the left hand side of (24a) can then be expected to exceed the other terms; if we accept this, we find:

$$n_1/n_2 = - m_2/m_1 \; ,$$

which becomes very small when m_2 refers to electrons. Equation (24a) for a = 2 (electrons) then can serve as a good approximation to the dispersion equation; we find :

$$\omega^2 \cong \frac{4\pi N_o e^2}{m_e} + i\omega\omega_{12} + \text{terms depending on the temperatures.}$$

The first term on the right hand side is the plasma electron frequency. Since this term determines the order of magnitude of ω^2, we can easily find that the waves with a wavelength large compared to the Debye length - which is a necessary condition involved in the Boltzmann equation (1) - the value of ω_2 will be large compared with unity, and this will hold a fortiori for ω_1 (see 22a).

This circumstance makes it possible to use the asymptotic expansion for Q_a :

$$Q_a = \frac{1}{\varpi_a^*} + \frac{1}{2\varpi_a^{*3}} + \frac{3}{4\varpi_a^{*5}} + \frac{15}{8\varpi_a^{*7}} + \ldots \tag{26}$$

Considerable simplification is possible in eqs. (25) and (24a). Equation (25) gives :

$$n_a = \frac{3}{2}\vartheta_a - \frac{3}{2}\, i\, \frac{\omega_{12}}{\omega}\,(\vartheta_a - \vartheta_{ab}) \,. \tag{27}$$

If there is no heat exchange between ions and electrons, we can make use of (15b), according to which $\vartheta_a - \vartheta_{ab} = 0$. We find then:

$$\vartheta_a = \frac{2}{3}\, n_a \,. \tag{27a}$$

When there is heat exchange, form. (27) makes it evident that there will be a phase difference between the oscillations in density and those in temperature.

The asymptotic expression for Q_a also leads to a simplification of eq. (24a), and we now obtain for the dispersion equation :

$$\begin{cases} \omega^2 = \dfrac{4\pi N_0 e^2}{m_e} + i\omega\omega_{12} + \\ + \dfrac{kT_0 \nu^2}{m_e}\left[1 + \left(3 + \dfrac{2i\,\omega_{20}}{\omega_2^*}\right)\dfrac{\vartheta_2}{n_2} - \dfrac{2i\,\omega_{12}}{\omega_1^*}\,\dfrac{\vartheta_2 - \vartheta_{21}}{n_2}\right]. \end{cases}$$

In the same assumption of absence of heat exchange between ions and electrons, this reduces to :

$$(28a)\quad \omega^2 = \frac{4\pi N_0 e^2}{m_e} + i\,\omega\omega_{12} + \frac{kT_0\nu^2}{m_e}\left(3 + \frac{4}{3}\,\frac{i\omega_{20}}{\omega_2^*}\right).$$

The result shows in the first place that there is a slight increase of the frequency above the plasma electron frequency, depending upon the wavelength $(2\pi/\nu)$ and the temperature of the gas. In the second place the formula brings into evidence the causes for damping of the waves. The main term in this connection is $i\omega\,\omega_{12}$, which indicates a damping upon the frequency of the electronic collisions; hence this term is connected with the resistance which the mean flow of the electrons experiences in consequence of the velocity difference with the mean flow of the ions. A much smaller source of damping is found in the last term on the right hand side, depending on the total collision frequency of the electrons and on the temperature and the wavelength; this is the damping due to the randomization of the velocities which occurs in collisions, but it is much less effective than the resistance effect due to the difference of velocity of electrons and ions. - It is interesting to observe that the ratio $i\omega_{20}/\omega_2^*$ changes from 0 for a very small collision frequency to -1 for a very high collision frequency.

(B) We can also assume that m_a is not large compared with $kT_0\nu^2$ (for neither value of $\underline{a}$). In this case we may expect $n_1 \cong n_2$, which means that there will be almost no charge separation, so that the electric field will loose much of its importance. We can now add the two equations (24a) for $a = 1$ and $a = 2$, and we obtain (neglecting the terms with $\vartheta_a - \vartheta_{ab}$):

$$(29) \qquad (m_1 + m_2)\omega^2 = kT_0 \nu^2 \left[2 + (3 + 2i\tilde{\omega}_{1o}Q_1) \frac{\vartheta_1}{n_1} + (3 + 2i\tilde{\omega}_{2o}Q_2) \frac{\vartheta_2}{n_2} \right] .$$

When the collision frequencies are sufficiently large to make $|\tilde{\omega}_a^*| \gg 1$ for both $a = 1$ and $a = 2$, so that we again can use the asymptotic expansion for Q_a, and when we use $\vartheta_a = (2/3)\, n_a$ and $2i\tilde{\omega}_{ao}Q_a = -1$, form. (29) reduces to:

$$(29a) \qquad \omega^2 = \frac{5}{3} \frac{kT_0}{1/2(m_1 + m_2)} \nu^2 .$$

Evidently we have fallen back upon acoustic waves.

12. It is of interest to consider the fluctuations of the pressure. If p_o is the partial pressure, either of the ions or of the electrons in the equilibrium case ($p_o = N_o\, k\, T_o$) , one immediately finds;

$$(30) \qquad (p - p_o)_a = N_o\, I\, T_o\, (n_a + \vartheta_a) .$$

One can also find the component p_{11} of the pressure tensor:

$$(31) \qquad \left\{ \begin{aligned} \frac{(p_{11} - p)_a}{p_o} &= 2\vartheta_a + \frac{2i\,\gamma_a}{\gamma C_a} Q_a = \\ &= (2 + 2i\,\tilde{\omega}_{ao}Q_a)\vartheta_a - 2i\tilde{\omega}_{ab}\, Q_a\, (\vartheta_a - \vartheta_{ab}) . \end{aligned} \right.$$

When the same results are used as before $\left[\vartheta_a \cong \vartheta_{ab}\; ; \vartheta_a = (2/3)n_a \right]$ this gives:

$$(31a)\qquad \frac{(p_{11} - p)_a}{p_o} = \frac{4}{3}\,\frac{\omega}{\omega - i\,\omega_{ao}}\,n_a \;.$$

Hence there is a marked pressure anistropy when the wave frequency ω is large compared with the collision frequency ω_{ao}, while the anistropy disappears when $\omega \ll \omega_{ao}$.

It is interesting to observe that the relations between the density increase, the increase of temperature and that of the mean pressure follow the adiabatic law with exponent 5/3 when the frequency is high and internal heat exchange is insignificant.

References and Notes

1) See addresses at the Dedication of the NOL Aeroballistic Research Facilities etc., June 27, 1949, NOLReport 1130, pp. 20 - 63, 96 - 98, issued Nov. 1949.

2) The work reported in this paper was supported by the Air Force Office of Scientific Research under Contract AF 49 (638)-401.

3) It will be evident that particles with a high relative velocity still may come so close together (when the direction of the relative motion is favorable) that an ordinary binary collision will result, practically uninfluenced by effects due to other particles. Such a collision could also lead to a reaction, when this is compatible with the nature of the particles.

4) Suppose that a particle $\underline{c}$ can result from a collision of particles $\underline{a}$ and $\underline{b}$ leading to a combination reaction (j), in which process the energy of the relative motion of $\underline{a}$ and $\underline{b}$ is stored at an unstable level. The particle $\underline{c}$ can either spontaneously dissociate back into particles $\underline{a}$ and $\underline{b}$, with a relaxation time τ; or it may happen to collide with a particle $\underline{d}$ in such a way that $\underline{c}$ passes into a stable state, while the excess energy is carried away as translational energy by $\underline{d}$ (there will also be a redistribution of momentum between $\underline{c}$ and $\underline{d}$). Using the notations explained in section 3 of the test, the following equation can be written down for $(dN_c)_j/dt$ (referring to the high energy stage) :

$$(dN_c)_j/dt = N_a N_b g_{ab} S_{ab} P_{ab/j} - N_c/\tau - N_c N_d g_{ab} S_{cd} P_{cd/\ell} ,$$

where $P_{ab/j}$ is the probability that the collision of <u>a</u> and <u>b</u> leads to a reaction j, producing a particle <u>c</u> in the high energy state; while $P_{cd/\ell}$ is the probability that a collision of <u>c</u> in the high energy state and <u>d</u> will lead to a reaction ℓ, bringing <u>c</u> into a stable state. If there is more than one kind of reaction which can do this, a summation with respect to ℓ would be necessary.

In an equilibrium state with $(dN_c)_j/dt = 0$, we find:

$$(N_c)_j = \frac{\tau N_a N_b g_{ab} S_{ab} P_{ab/j}}{1 - \tau N_d g_{cd} S_{cd} P_{cd/\ell}} .$$

In many cases it may be possible to neglect the second term in the denominator; this will occur when the relaxation time is very short. The number of stable particles formed in unit time per unit volume is then:

$$(dN_c)_\ell/dt = N_a N_b g_{ab} S_{ab} P_{ab/j} \cdot \tau N_d g_{cd} S_{cd} P_{cd/\ell} .$$

If we consider $\tau g_{cd} S_{cd} P_{cd/\ell} = \Omega_\ell$ to define a "sensitive volume", the result can be written in the form:

$$(dN_c)/dt = N_a N_b N_d g_{ab} S_{ab} P_{ab/j} \Omega_\ell ,$$

which is the form for a reaction in which triple collisions are involved.

5) Formally one can also speak of a distribution function for the photons of a particular radiation frequency. Here the absolute value of the velocity is the same for all photons, but the distribution over the directions in space is of importance. Starting from the expression for the intensity of radiation $\mathcal{J}(\omega, \underset{\sim}{\alpha})$, defined in such a way that $\mathcal{J}(\omega, \underset{\sim}{\alpha})\, d\omega\, d\Omega$ is the flow of radiative energy within an elementary cone $d\Omega$ around the direction $\underset{\sim}{\alpha}$, and within the frequency interval $d\omega$, in unit time through unit area perpendicular to $\underset{\sim}{\alpha}$, the distribution function is obtained as:

$$\mathcal{J}(\omega, \underset{\sim}{\alpha})\, d\omega\, d\Omega / \hbar\omega ,$$

where $\hbar\omega = h\omega/2\pi$ is the energy of a photon of frequency ω.

6) This probability law is not always given explicitly, but it serves for the calculation of cross sections occurring in connection with the standard expression for binary collisions. Compare e.g. S. Chapman and T. G. Cowling, The Mathematical Theory of Non-uniform Gases (Cambridge, England 1939 and 1952), Chapter 10; or J. O. Hirschfelder and others, The Transport Properties of Gases and Gaseous Mixtures, Section D of "Thermodynamics and Physics of Matter", ed. by F. D. Rossini (Princeton, N. J., 1955), pp. 348 and foll.

7) See M. Krook, Dynamics of Rarefied Gases, Phys. Rev. 99, pp. 1896-1897, 1955; E. P. Gross and M. Krook, Model for Collision Processes in Gases: Small-amplitude Oscillations of charged Two-component Systems, Phys. Rev. 102, pp. 593 - 604, 1956.

8) We shall come back to this point in Example III (see eqs. 15a, 15b).

9) See: M. Born and H. S. Green, A General Kinetic Theory of Liquids. I. The Molecular Distribution Functions, Proc. Roy. Soc. London A 188, pp. 10 - 18, 1946. - H.S. Green, Molecular Theory of Fluids (Amsterdam 1952), p. 128, eq. (2.7) etc.

J. G. Kirkwood, The Statistical Mechanical Theory of Transport Processes. I. General Theory, J. Chem. Phys. 14, pp. 180 - 201, 1946; II. Transport in Gases, ibidem 15, pp. 72 - 76, 1947.

N. Bogoliubov, Kinetic Equations, Russ. J. of Physics 10, pp. 265-274, 1946; Problems of a Dynamical Theory in Statistical Physics, Transl. by E. K. Gora, Geophysics Research Directorate, Air Force Cambridge Research Center, Jan. 1959.

10) S. W. Temko, On the Deduction of a Fokker-Planck Equation for a Plasma, Russ. J. Exper. Theor. Phys. 31, pp. 1021 - 1026, 1956 (Amer. Instit. of Physics Translation in "Soviet Physics" JETP 4, pp. 898 - 903, 1957).

11) C. M. Tchen, Kinetic Equation for a Plasma with Unsteady Correlations, Nat. Bureau of Standards Report 6274, Washington, D. C. 1958.

One of the main points of this paper is the passage from the function $Y_0(\nu) = (2\pi^2\nu^2)^{-1}$, which is the Fourier transform of the Coulomb potential, to the function $Y_{\kappa}(\nu) = (2\pi^2)^{-1}(\nu^2 + \kappa^2)^{-1}$, characterizing the Debye potential (1. c. p. 19).

12) S. Gasiorowicz, M. Neumann and R. J. Riddle, Jr., Dynamics of Ionized Media, Phus. Rev. 101, pp. 922- 934, 1956.

13) Tchen's assertion that the disturbances associated with the correlation function provide cooperative phenomena of such a type as to assure convergence at close interaction is not proved, since the quantity Δ_0 defined in (23c), p. 21, can become negative for high values of ν (the quantity ν^* defined in B 12, p. 49, depends upon the sum $e_a \sum N_b e_b^3$ and can have negative as well as positive values).

14) All probabilities considered here are ensemble probabilities.s

15) These distances are of the order of the so-called Debye screening length, which is the inverse of the quantity κ defined by (13).

16) The assumptions embodied in eqs. (9) and (10) and the introduction of eqs. (11) and (12) owe their origin to a translation into coordinate language of certain results obtained by Tchen from the Fourier analysis.

17) It is this assumption which introduces the irreversibility with respect to the time into the treatment. Compare H. S. Green, Molecular Theory of Fluids (Amsterdam 1953), p. 216.

18) Compare Tchen, 1. c. (ref. 11), p. 23.

19) Compare H. S. Green, Molecular Theory of Liquids (Amsterdam 1952), pp. 218 - 225. See also Chapter III, section 19, of the proceedings Intern. Symposium on Plasma Dynamics, Woods Hole, Mass., June 1958 (to be published by Addison-Wesley Publishing Co., Inc.)

20) Much of the treatment given here runs parallel to that presented by E. P. Gross and M. Krook, Phys. Rev. 102, pp. 593 - 604, 1956.

SURVEY OF NOL AERODYNAMIC RESEARCH

1949 to 1959

by

Dr. R. Kenneth Lobb
Chief, Aerodynamics Department
U. S. Naval Ordnance Laboratory

Introduction

Here we are at the end of our first decade and at the beginning of the second. It now appears to be timely to look back upon some of the accomplishments of the Aerodynamics Department and forward to some of the future possibilities.

Ten years ago we were just starting the shakedown of the refurbished German wind tunnels. At that time we were also laying the groundwork for a long-range research program on aerodynamics. The emphasis of this program was directed towards the supersonic and hypersonic speed ranges. In still another area, we were planning and proposing the expansion of our facilities to cover the Mach number range between 5 and 10.

Now as we look back on 1949, we see that many of our hopes and expectations have been more than fulfilled. During this decade we have seen the velocity of vehicles increase from the transonic range up to and beyond satellite speeds. The Aerodynamics Department of NOL has played an important role in this advance, both in the developmental area and in fundamental research. In addition, we have greatly improved the quality of the flow in the wind tunnels and have added new facilities to increase the operating range up to Mach number 10.

In this review paper I will discuss first the aerodynamic and mechanical development of the NOL hypersonic wind tunnels. This will be followed by a review of the highlights of our research program for the past ten years. Finally, I will say a few words about the developmental tests on hardware.

Hypersonic Wind-Tunnel Development

The Naval Ordnance Laboratory has had a small hypersonic wind tunnel operating in the Mach number range from 5 to 10 since May 1950. The original version of this tunnel had nozzle exit dimensions of 12 x 12 cm. Recently, it was modernized to accommodate 10 x 10-inch two-dimensional and 12 1/2-inch diameter axisymmetric nozzles. The results obtained in this small tunnel have been extremely valuable to both the wind-tunnel designer and the high-speed missile designer.

Air Liquefaction

Perhaps the most widely known investigation carried out in this facility was the study of air liquefaction in hypersonic nozzles at Mach numbers up to 10. These results demonstrated very clearly that air liquefaction occurs slightly below the saturation line for air and that high supply temperatures were not necessary for high Mach number operation. Certainly the NOL data, along with some of that of NASA, CIT, and APL, helped to clear this misconception. The first figure gives some indication of the early results. Here we have Mach number plotted as a function of the stagnation temperature. The Mach number is determined by three methods: First, the ratio of Pitot to supply pressure, then static to supply pressure, and finally from Rayleigh relationship. You can see that when the air temperature is considerably below the saturation line for air then these three fictitious Mach numbers give greatly different answers, but as the temperature approaches the saturation line for air the three Mach numbers are in good agreement.

Diffuser Study

After this initial program, we conducted an extended developmental test on a variable area diffuser for hypersonic tunnels. This was in response to a request from the Gas Dynamics Facility of the AEDC. The results gave more favorable pressure ratios than known before and therefore the power requirements for starting and running hypersonic tunnels were lower than was originally expected.

Nozzle Heat Transfer

One of the major problems in tunnel research and development was to overcome the high wall temperatures resulting from high supply temperatures. As early as 1951, calculations of heat transfer to the nozzle surface were made for laminar and turbulent boundary layers including the effects of pressure gradient. It was found that a pressure gradient increases the heat transfer near the throat many-fold. The over-all heat transfer was measured by three different methods and found to be in good agreement with the theoretical predictions. These results were very important for the design of hypersonic tunnels in all three services. The second figure presents some of the early results which we obtained on the aerodynamic heating of nozzle throats. Here you see we have the heat-transfer rate to the nozzle surface plotted as a function of the distance along the nozzle. In both cases where we have a laminar flow and turbulent boundary-layer flow, the heat transfer peaks very rapidly at the throat. These experimental results are in good agreement with the theoretical predictions. An interesting point here is that the heat transfer to the nozzle throat in the new Hypersonic Tunnel No. 8 (where the pressures are of the order of 150 atmospheres) is roughly equivalent to the rate of heat transfer at the stagnation point of a ballistic missile.

Instrumentation

In addition to the developments which I have just described, a wealth of knowledge has been accumulated on special hypersonic instrumentation such as water-cooled strain gage balances, Pitot and static pressure probes, mass flow probes, and precision manometers which measure in the range from 0 to 40-mm of mercury with an accuracy of roughly 1 micron. We have also developed small total temperature probes which can be used to survey the boundary layers and flow fields in hypersonic wind tunnels.

NOL Hypersonic Tunnel No. 8

The Hypersonic Tunnel No. 8 is being developed for large scale testing of hypervelocity vehicles in the Mach number range 5 to 10. It is a blowdown system operating from high pressure storage bottles and exhausting into the atmosphere. At the present time the final components of the facility are being installed. It is expected that shakedown operations will begin in the very near future.

A cutaway view of the tunnel is shown in Figure 3. The high pressure supply consists of 40,000 pounds of air stored at 3,000 P.S.I. and 5,000 P.S.I. Pressure regulating valves control the stagnation pressure of the tunnel in the range from 1 to 150 atmospheres. A storage heater containing silicon iron balls and a topping electric heater maintain the stagnation temperature at a constant value in the range from atmospheric temperature to 2,000°R. (Heating of the air is, of course, necessary if air condensation in the test section is to be avoided.) The air is expanded in 18 x 20-inch uniform flow two-dimensional nozzles for Mach numbers up to and including 8. A 25-inch axisymmetric nozzle will be available for testing at Mach number 10. All nozzles have a linear correction for the boundary-layer displacement thickness. In the case of the two-dimensional nozzles, the sidewalls are diverged and the nozzle walls are opened up to provide for this correction. Downstream of the nozzle and test section the air is decelerated in a convergent divergent diffuser. All of the working section components which are exposed to the airflow are water-cooled in order to keep the thermal stresses and distortions to a minimum. At the end of the diffuser the air exhausts into the atmosphere either directly or else via an aftercooler and a 10,000-horsepower vacuum plant.

A brief summary of the operating characteristics of the tunnel is given in Table I.

Some Research Results

I would now like to say a few words about our research program which we have been conducting over the past ten years.

Base Pressure

One of the first programs to be carried out by this group was an experimental study of the base pressure on axisymmetric bodies at supersonic speeds. These results, along with those from the NASA, demonstrated very clearly that the base pressure is not only a function of the Mach number and free-stream Reynolds number, but is also very much dependent on the local boundary-layer conditions and local free-stream properties near the base of the body. These tests were carried out on sharp-nosed bodies with and without boat-tailing.

More recently we have extended this investigation to higher Mach numbers and also to blunt-nosed hypersonic bodies. A typical base pressure curve for laminar, transitional, and turbulent boundary-layer flows is shown in Figure 4. Here we see that a single curve can be used to correlate the data from two different body shapes. Actually the only difference between these two configurations is that one has a spherical nose, whereas the other has a sharp point. What this test essentially demonstrates is that if you have the same flow conditions just forward of the base (that is, the same Mach number, the same Reynolds number, and the same boundary-layer characteristics) then the base pressures will be identical. In order to achieve these identical conditions, we adjusted the free-stream Mach number and Reynolds number for each shape. For example, a Mach number 5 nozzle was used for the blunt cone, whereas a Mach number 2.8 nozzle was used for the sharp cone.

Blunt Body Heat Transfer

Perhaps the most significant investigation to be conducted by our research group during the past ten years is that dealing with the aerodynamic heating and flow field studies for blunt-nosed bodies. This research was started in 1950 and the first important result to come out of it was the publication of the now classical paper in the Journal of the Aeronautical Sciences entitled "Heat Transfer Near the Forward Stagnation Point of a Body of Revolution". Now when you think back to 1950 and 1951, you will remember that there was a general lack of interest in hypersonics and certainly a great dislike for any blunt body with a high drag. Even so, we could see the future possibilities for these shapes and so we continued with this research.

Now at that time we were not only interested in the heat transfer to bodies with solid surfaces, but we were also conducting feasibility studies on ablation and mass transfer. Figure 5 shows an ablating ethylene chloride sphere mounted in Supersonic Tunnel No. 1. This study was made in 1951 and led eventually to the more up-to-date mass transfer investigations which we are currently conducting. Even so, these early results provided many of the qualitative answers to several of the then unsolved problems on ablation.

Returning now to the heat transfer on solid bodies, I would like to show you a typical comparison of the experimental data with the theoretical predictions. All of the data to be shown are for a perfect gas flow. Figure 6 gives this comparison. Here we have plotted a dimensionless heat-transfer parameter as a function of the distance along the hemisphere. You can see that the theoretical predictions for the stagnation point and also around the hemisphere are in good agreement with the experimental measurements. The experimental measurements were made in the wind tunnel and the shocktube. Also shown on this figure is a typical curve for turbulent boundary-layer heat transfer. It can be seen that the heat transfer for turbulent flow rises rather rapidly. This is the type of heat transfer which you would experience on the forward part of a high-speed re-entry vehicle when it penetrates the denser part of the atmosphere.

I should point out that the simplified theoretical treatment which is shown on this figure has been extended both here at NOL and at other agencies to include the real gas thermodynamic and transport properties. These refined calculations are extremely valuable for detailed studies on a specific body configuration. However, for a parametric study of several body shapes the simplified theory developed earlier is still extremely useful. By this I mean that it provides the trend and in most cases it gives a satisfactory engineering answer.

Pressure Distribution on Blunt Bodies

Another interesting feature of the early tests on blunt-nosed bodies was the pressure distribution measurements. The data showed the very surprising result that the Newtonian flow concept was quite adequate for predicting the pressure distribution over most of the hemisphere. Now again you must think back to the early 1950's. In those years there was no theoretical or experimental information that could be used by the high-speed missile designer to predict the flow field and boundary-layer growth over these blunt bodies. More recently several theoretical methods have been developed which confirm the experimental observations. It is instructive to remember, however, that in the early days of the ballistic missile development the designer had to rely entirely on experimental measurements of the type that were obtained here.

Turbulent Boundary-Layer Theory

Shortly after undertaking the blunt body research, it became evident that there was a great need for theoretical and experimental information on turbulent boundary layers, particularly at hypersonic speeds. The theoretical treatment which was developed at NOL was aimed primarily at providing a method for predicting the aerodynamic heating, skin friction, and other boundary-layer characteristics on hypersonic bodies of arbitrary shape. The procedure which was programmed for the high-speed computing machines has been used extensively over a number of years to predict the aerodynamic heating of many free-flight vehicles.

Turbulent Boundary-Layer Measurements

The experimental measurements of turbulent boundary-layer characteristics which went along with the theoretical development were made in the supersonic and hypersonic wind tunnels. The most important of these investigations was a detailed survey of the turbulent boundary layer on the nozzle wall of our small hypersonic wind tunnel. These measurements were carried out in the Mach number range of 5 to 8.2. The results provided some of the very first accurate values of skin friction and heat-transfer coefficients at hypersonic speeds. They also verified for the first time the applicability of Reynolds analogy between heat transfer and skin friction at hypersonic speeds.

As I mentioned a few moments ago all of these data were obtained on the nozzle wall and in a flow with a slightly favorable pressure gradient. In order to eliminate any pressure gradient or history effects, a new series of measurements were made on a smooth flat plate mounted in the free stream. These new tests were also designed to provide a much wider range of heat-transfer conditions. Figure 7 shows a summary of these results. On the ordinate we have plotted the local skin-friction coefficient and on the abscissa we have plotted the local Reynolds number based on the boundary-layer momentum thickness. The three curves are for three different rates of heat transfer. Actually the rate of heat transfer to the wall increases as we proceed downward in the figure. Perhaps the most significant result presented here is that the skin friction and heat-transfer coefficients decrease as the heat transfer to the wall increases. This trend is in the opposite direction to that predicted by the theory. It also indicates the great need for a better physical picture of the boundary-layer structure so that a more adequate semi-empirical theory can be developed.

Correlation with Other Data

As a result of an effort to compare these data with the results of other experimental investigations we have come up with the correlation shown in Figure 8. Here the straight line is a simple incompressible theory given by the equation

$$C_{f_i} = 0.0246\, Re_\theta^{-0.251}$$

Where C_{f_i} is the incompressible skin-friction coefficient and Re_θ the Reynolds number based on boundary-layer momentum thickness. The experimental points represent "adjusted" data taken from the references given at the bottom of the figure. In order to make this adjustment we have multiplied the experimental skin-friction coefficient by a Mach number adjustment

$\left(\frac{T_o}{T_\infty}\right)^{1/2}$ and also by a heat-transfer adjustment $\left(\frac{T_e}{T_{wall}}\right)^{1/4}$

If we correct the data in this manner then we find that it all collapses on top of the incompressible skin-friction curve. With this result available, we can now write down the following relationship for predicting the turbulent boundary-layer skin-friction coefficient as a function of Mach number and rate of heat transfer

$$C_{fc} = C_{fi} \left(\frac{T_\infty}{T_o}\right)^{1/2} \left(\frac{T_w}{T_e}\right)^{1/4}$$

where C_{fc} is the compressible skin-friction coefficient, T_∞ the local free-stream temperature, T_o the stagnation temperature, T_w the wall temperature, and T_e the equilibrium recovery temperature. This simple result is quite useful when you consider that all of the turbulent boundary-layer theories are at best semi-empirical. Consequently, our attitude is that you may as well use the simple relationship given above to predict the skin friction rather than use the more involved empirical theories.

Laminary Sublayer

Another interesting feature of the turbulent boundary layer is the rather thick laminar sublayer which exists at hypersonic speeds. This is demonstrated in Figure 9. Here I have plotted the thickness of the laminar sublayer divided by the total thickness of the turbulent boundary layer as a function of the Mach number. The curve is derived from a semi-empirical theory based on data for flows with zero heat transfer. It can be seen that the experimental data points have the same general trend as the theoretical predictions. The scatter in the data points was largely due to the variation in heat transfer and Reynolds number.

With these data available it is interesting to speculate on the thickness of the laminar sublayer when the local Mach number has a rather high value of the order of Mach number 15. It certainly seems very probable that in such a case the laminar sublayer could easily occupy half of the total boundary-layer thickness.

Laminar Boundary Layer with Real Gas

Earlier in my talk when I was discussing the heat transfer to the stagnation point on blunt-nosed bodies, I mentioned that we have developed a new theory which takes into account more fully the local flow conditions behind the shockwave, including the real gas effects. Actually this theory has a much wider area of application than for the stagnation point alone. In fact, we have programmed a procedure on the IBM 704 computer which includes the full two-thickness momentum and energy integral equations for the real gas laminar boundary layer. Solution of the momentum and energy equations are conducted simultaneously which avoids recourse to the use of Reynolds analogy in determination of the heat transfer. In addition to the inclusion of real gas thermodynamic and transport properties, the procedure includes the effects of variable body surface temperature and pressure gradient. I should add here that the real gas properties have been obtained from the data supplied by the National Bureau of Standards.

Hardware Development

In conclusion I would like to discuss briefly a few of our developmental programs which have rendered important services to the missile designer.

Magnus Measurements

First there is the Magnus force and moment measurements on spinning bodies in our supersonic and hypersonic wind tunnels. These measurements have proved very helpful to the missile designer and have made it possible for the first time to predict and prevent the erratic motion of many of the unguided missiles.

Damping in Pitch

To mention another area there are the damping in pitch measurements which have been made for many years on freely oscillating models. The technique used here is to mount the model on a strut through the center of gravity and then record the oscillating motion as a function of time. From the record of this motion we can then determine the damping in pitch of the model under investigation. Such measurements have proved very helpful in the design of many unguided missiles such as bombs and reentry vehicles.

Stability at Hypersonic Speeds

Still another developmental area which has been quite fruitful is the measurement of static forces and moments in the hypersonic wind tunnel. One of the significant results to come out of these tests was that the static stability of blunted slender bodies at hypersonic speeds falls off very rapidly with increasing Mach number. I might add that this has confirmed the theoretical predictions.

I have given here only a few examples from our developmental test programs. Figure 10 gives a much better picture of the type of vehicles which have been tested in the wind tunnel. Here you can see that most of our testing has been devoted to Navy missiles. However, I think it is quite apparent from this figure that some of our good customers have been the Army and the Air Force missile groups. Actually there are many other missiles and airplanes which I have omitted due to space limitations or else due to security measures.

As a last word I should like to say that the accomplishments which have been outlined here this morning represent the contributions of many scientists working over the years at NOL. It was indeed very pleasant for me to have an opportunity to present the results to you here this morning.

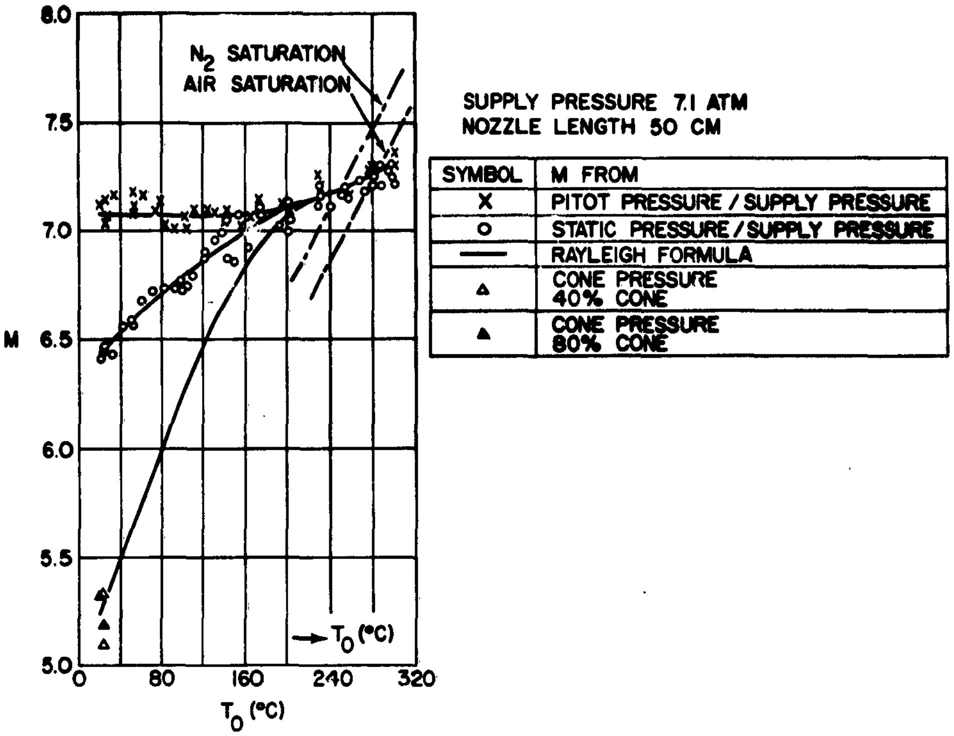


Fig. 1 Variation Of Mach Number With Stagnation Temperature

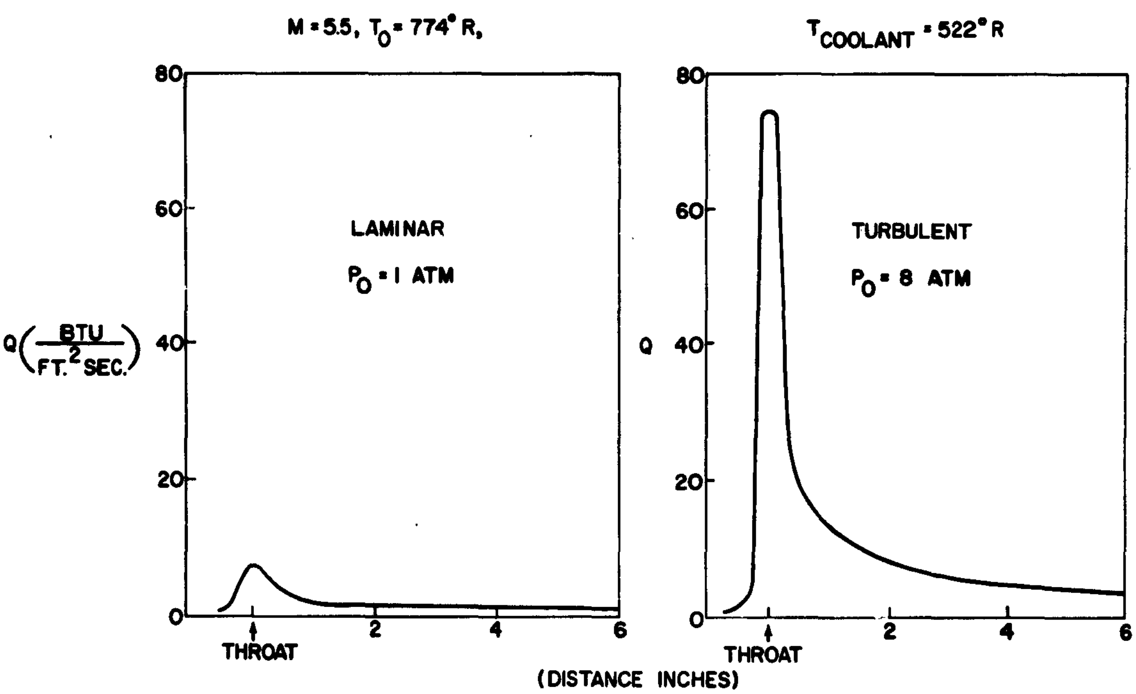


Fig. 2 Heat Transfer To Nozzle Surface

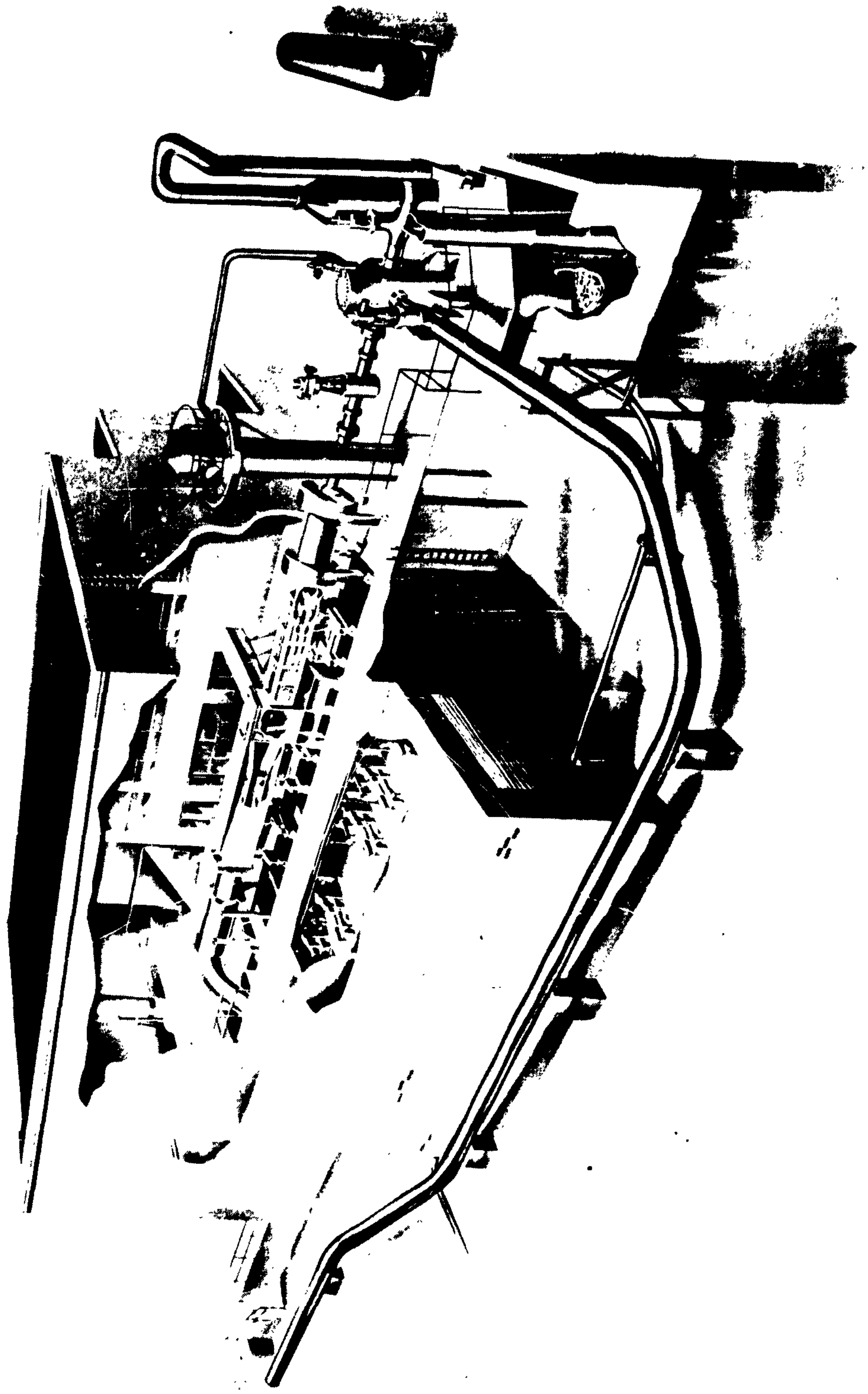


Fig. 3 The NOL Hypersonic Tunnel No. 8

TABLE I

Operating Conditions for the NOL Hypersonic Wind Tunnel No. 8

Mach No.	Stagnation Pressure	Stagnation Temperature	RE/ft $\times 10^{-6}$	Discharge Rate Lbs/Sec	Heating Power Req. (BTU/Sec) or KW
5	100 atm	751°R	51.8	292	21,200
6	150	967	34.4	181	22,900
7	150	1177	17.4	84	15,000
8	150	1410	9.4	42	9,900
9	150	1666	5.4	22	6,800
10	150	1936	3.2	13	4,700

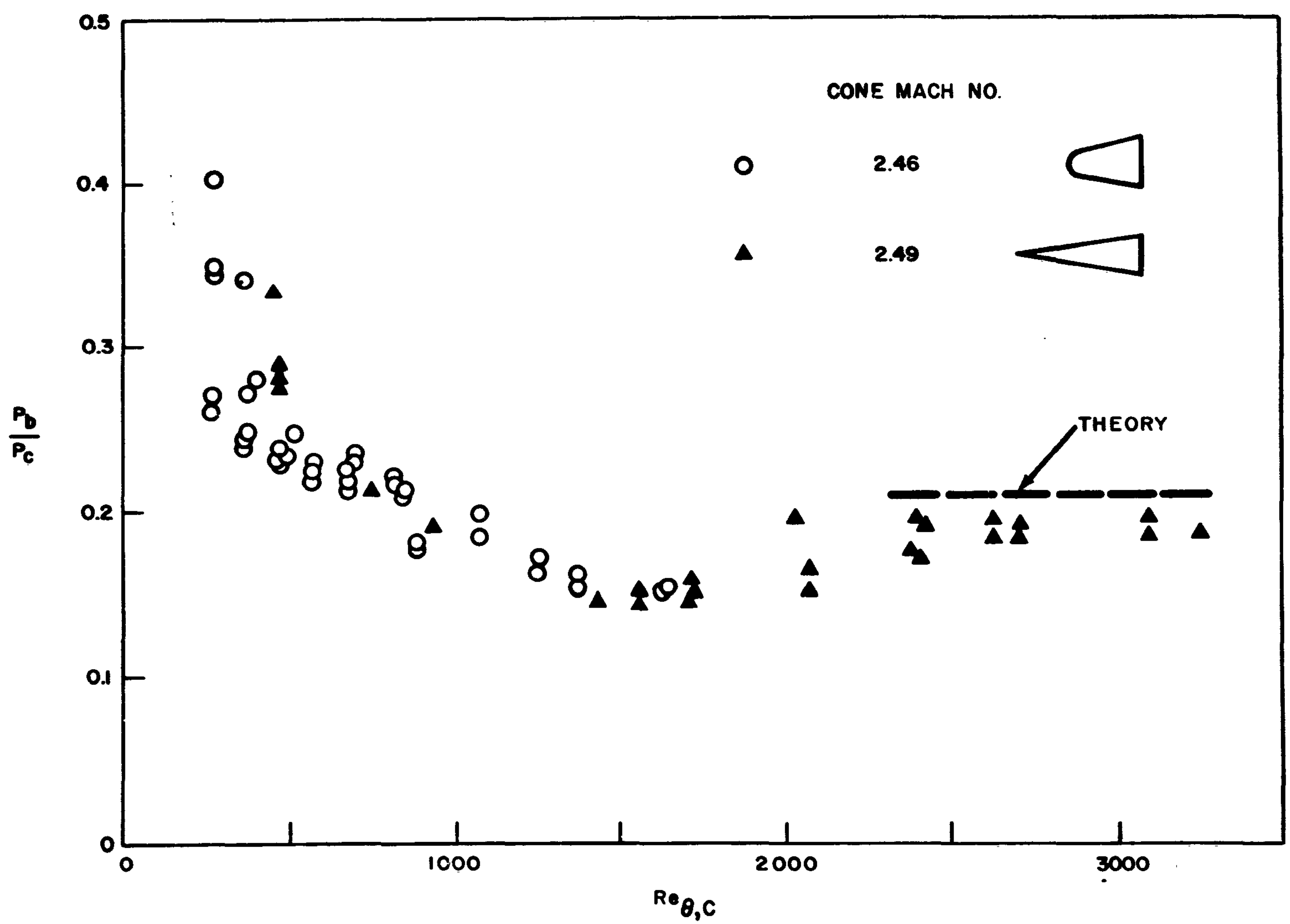


Fig. 4 Variation Of Base Pressure With Reynolds Number (Re_e)

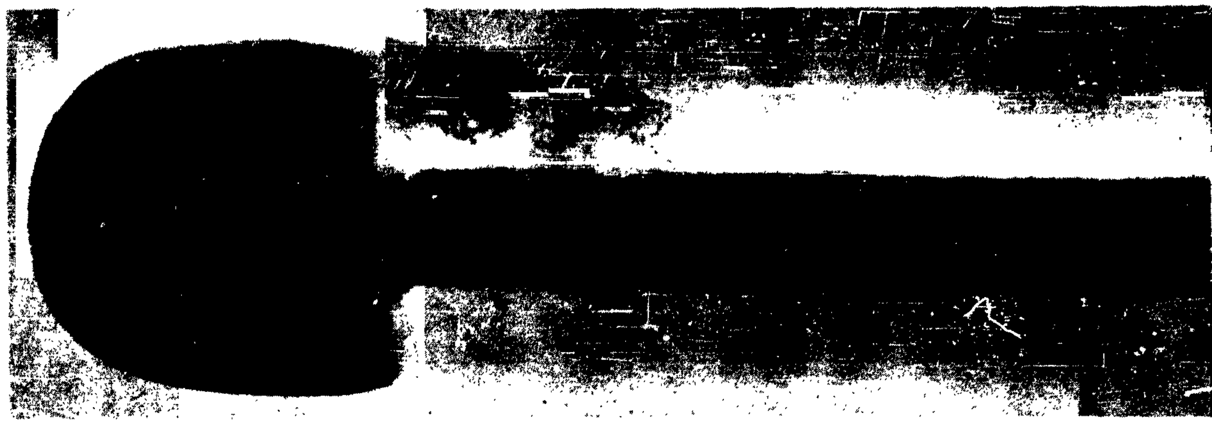
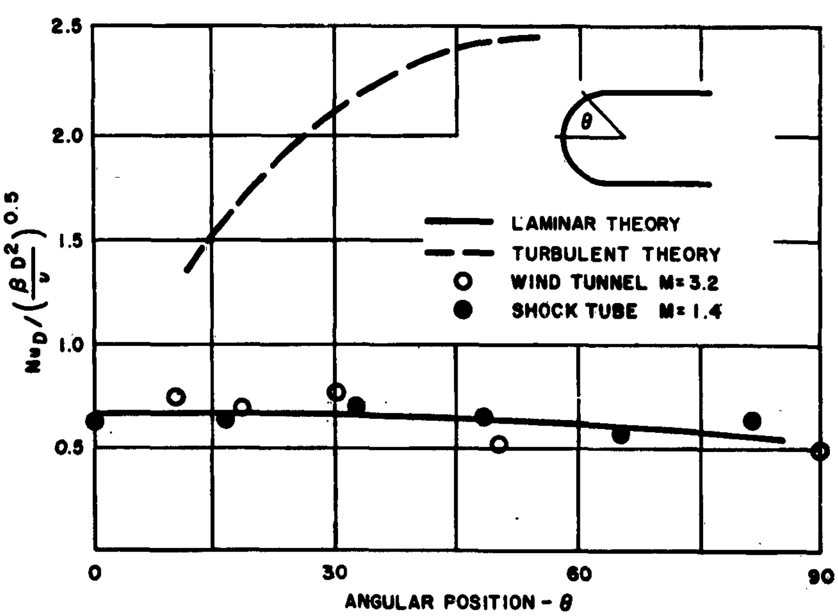


Fig. 5 Photograph Of An Ablating Ethylene Chloride Model

FIG. 5 PHOTOGRAPH OF AN ABLATING ETHYLENE CHLORIDE MODEL



Nu_D - NUSSELT NUMBER BASED ON BODY DIAMETER
β - LOCAL VELOCITY GRADIENT EVALUATED AT STAGNATION POINT
ν - KINEMATIC VISCOSITY COEFFICIENT
D - BODY DIAMETER

Fig. 6 Laminar And Turbulent Heat Transfer To A Hemisphere

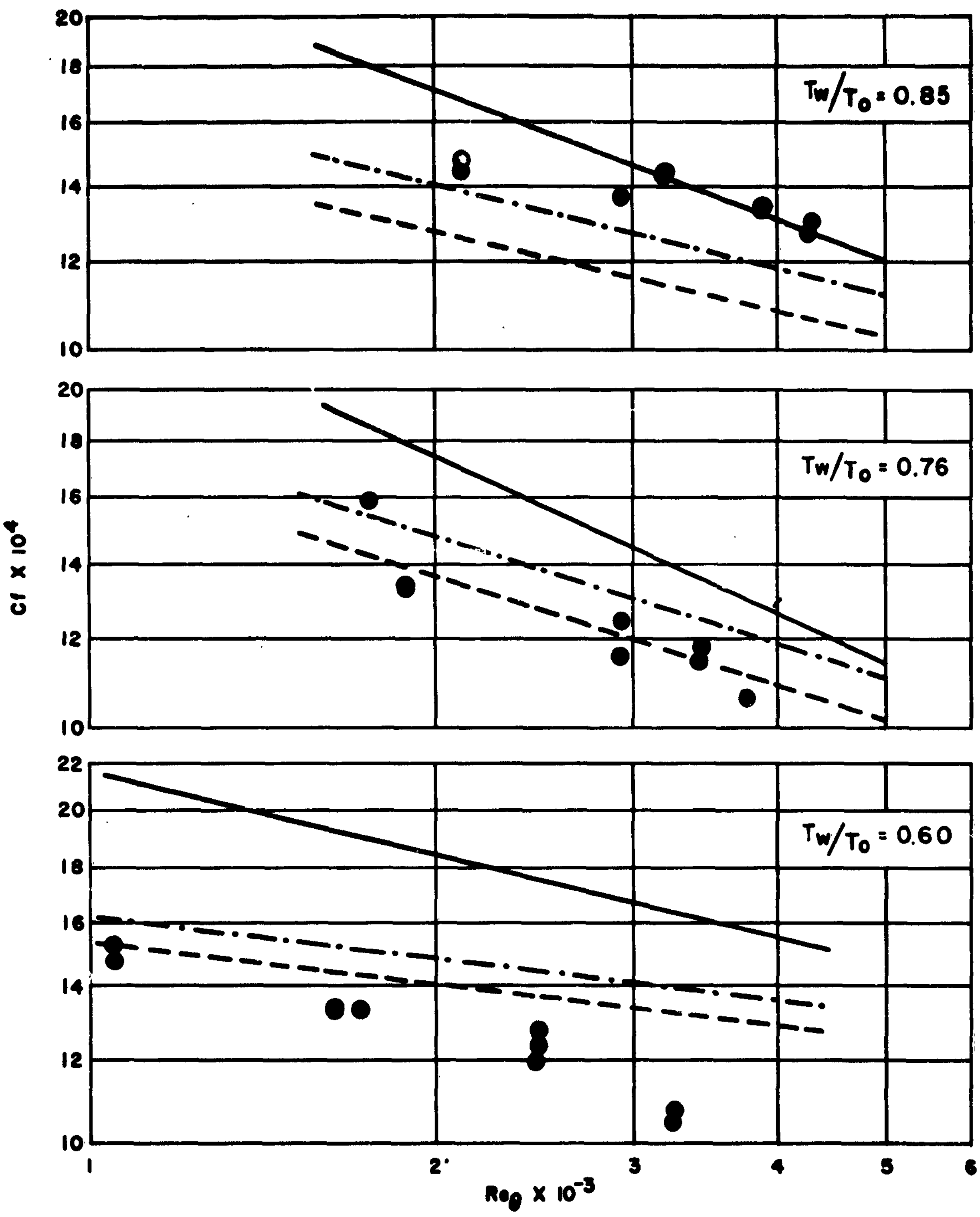


Fig. 7 Variation Of Local Skin Friction Coefficient With Mach Number

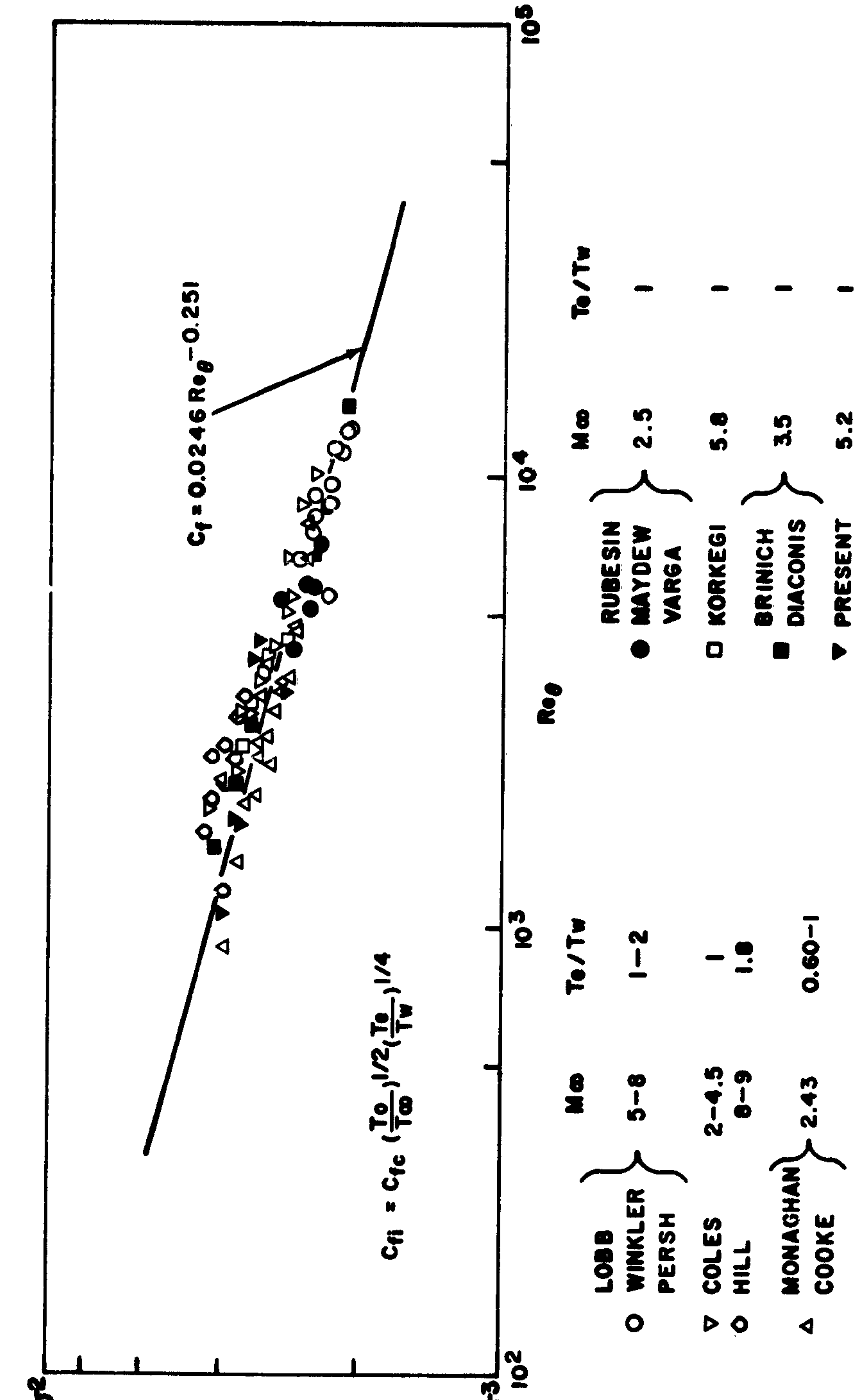


Fig. 8 Correlation Of Experimental Skin Friction Data

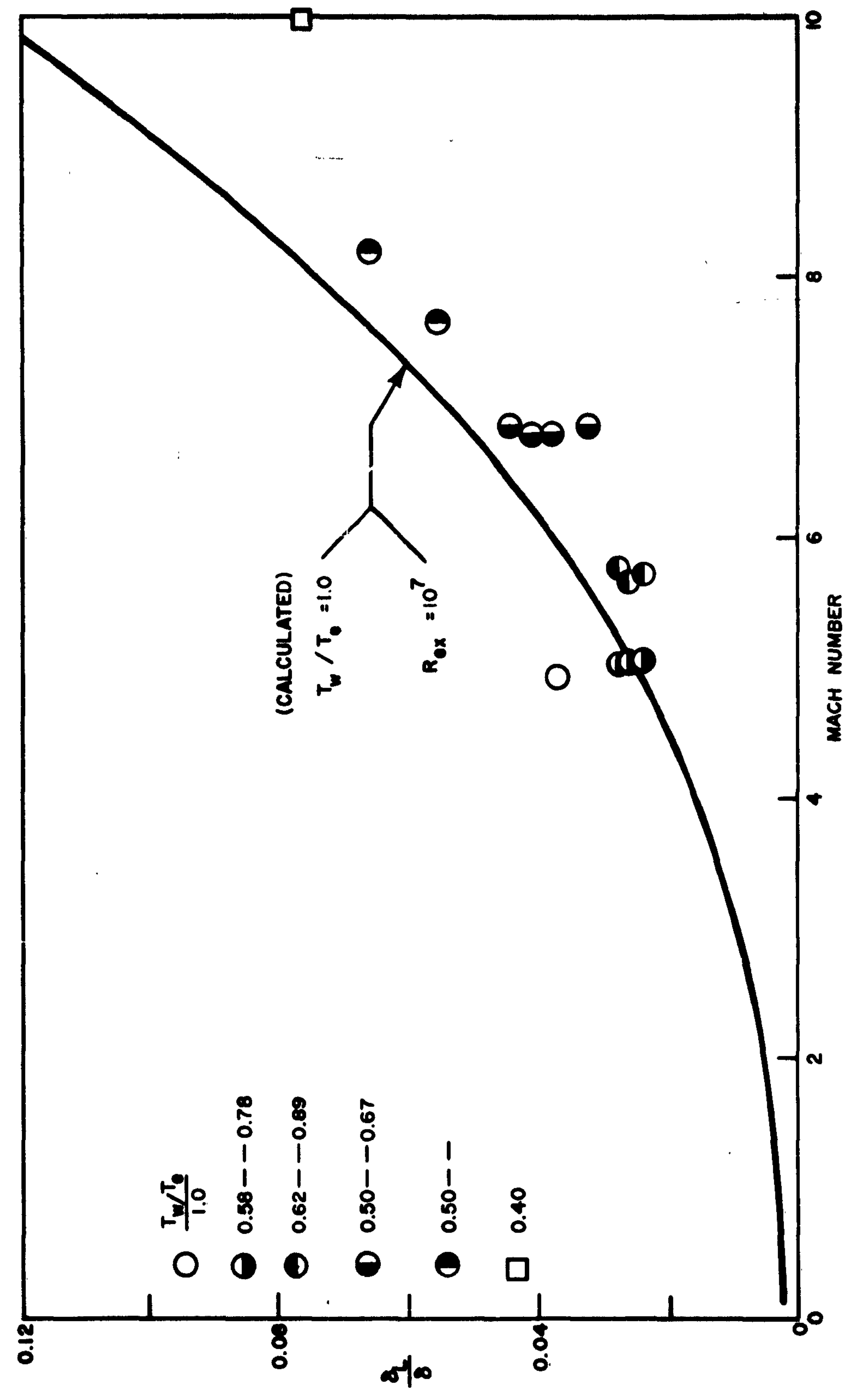


Fig. 9 Variation Of Laminar Sublayer Thickness With Mach Number

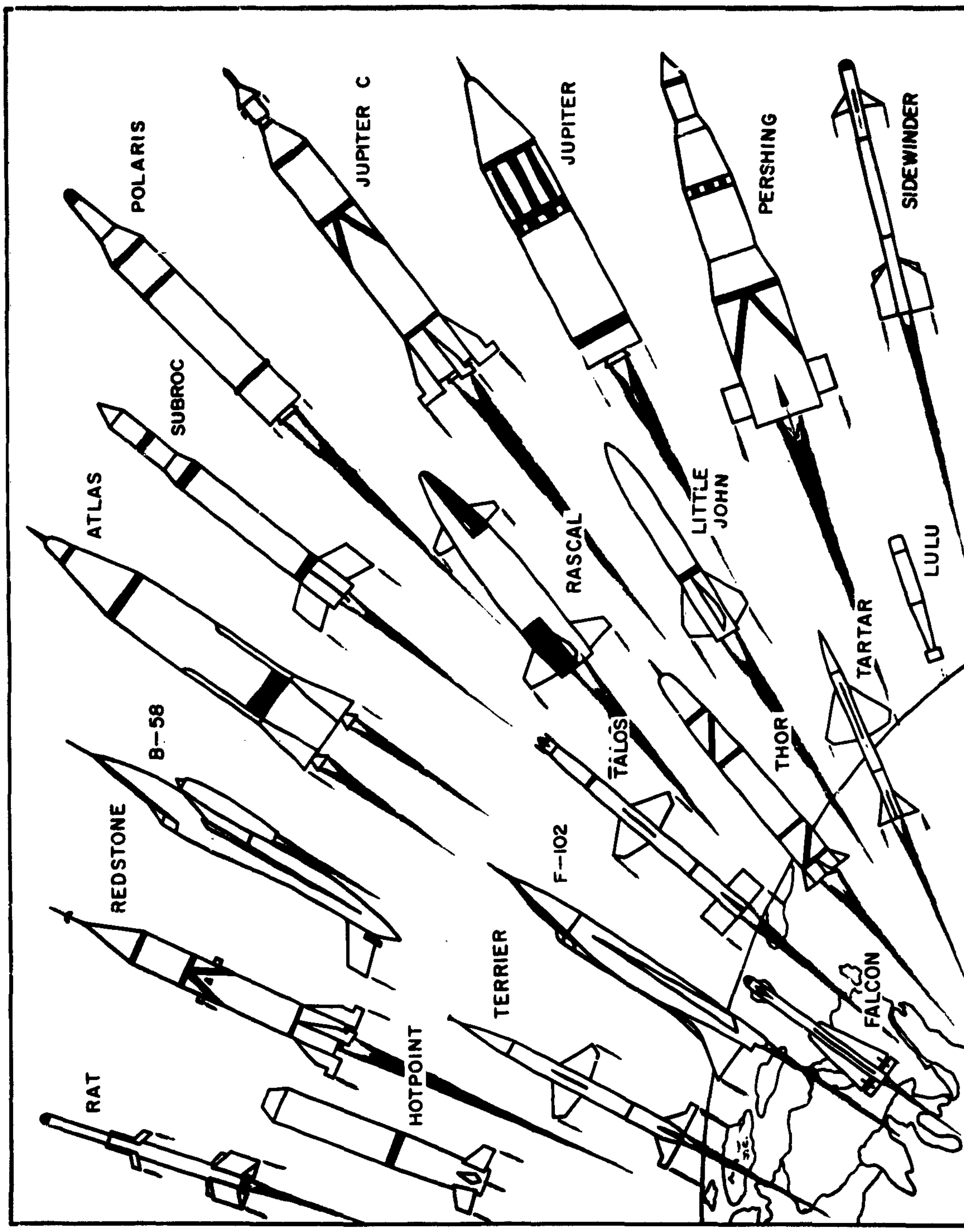


Fig. 10 Some Missiles That Have Been Tested In The NOL Wind Tunnels

SURVEY OF NOL HYPERBALLISTIC RESEARCH

by

Dr. Z. I. Slawsky
Chief, Ballistics Department
U. S. Naval Ordnance Laboratory

Ladies and Gentlemen: this is a symposium celebrating the decennial of the establishment of the Aeroballistic Facilities and the dedication of those which have just come into existence. I thought it would not be too far out of place for me to review, therefore, some of the work that the personnel of the Ballistics Department had been involved in during the last ten years and the contributions they have made in this field. The work that I am going to report on right now falls more or less into four broad areas where we have, we think, contributed ideas. The first of these is the gas dynamics of highly compressed gases. The second area is the gas dynamics of high temperature gases. The third area is the research that goes into the development of high-speed model launchers. The fourth is the research that goes into the development of high-temperature, high-speed, high Mach number flow generators, which we now know as shock tunnels. I would like to tell you a little about these four areas.

Obviously, all I can do is present briefly some of the ideas that we worked on and some of the results that we obtained. I will not go into detail on any of them; I hope that we have a chance to discuss them in detail at some other time. Most of this work is published.

Section I

Early in 1949, we became interested in the effect of intermolecular forces on the efficiency of the gas in accelerating pistons in the tube, as occurs in guns. Until that time, the effect of sound speed was already somewhat understood although not indicated in published literature. However, the effect of the attractive forces between molecules, i.e., condensation forces, and the repulsive forces was not discussed in the literature and the separate effects were not investigated in any systematic way.

A theoretical and experimental program was begun. Calculations were made for the simple-wave type of expansion of a nonideal gas, not only one that obeys the Van der Waals equation of state, but also for real nitrogen, one for which the equation of state was given graphically. A calculation was carried through for highly compressed nitrogen up to 3000 atmospheres. It became evident at once that a piston which is accelerated by compressed nitrogen gave a velocity history measurably different from that to be expected under the assumption that $PV = RT$ is adequate as an equation of state for this gas. In fact, the inclusion of a co-volume correction which is the one that is ordinarily put into theoretical treatment of propellants, viz., $P = (V-b) = RT$, gave a result which was the same as if b were equal to zero. Together with this theoretical work, a series of experiments was undertaken. An apparatus was built at the Van der Waals Laboratory in Amsterdam. A set of experiments was begun in which nitrogen was compressed into a chamber the diameter of which was equal to the diameter of the tube itself. Accurate measurements were made on the position-time history of a piston in the tube.[1,2,3,4] The confirmation of the theory was quite gratifying; the accurate determination of the acceleration of the piston gives information on the equation of state of the gas.

In 1952, this work was extended to include the effect of finite chambers and chambers the diameter of which was greater than the bore diameter of the tube. This is the effect known as chambrage.

The theoretical calculations were carried through and a measurable effect was evident. As a matter of fact, it turns out that if the ratio of the chamber diameter to the gun diameter is sufficiently large, the muzzle velocity is increased by about one-half the sound speed of the propellant.[5] If, therefore, the propellant has a high sound speed, the improvement is substantial. This theoretical work was tested by a series of experiments. The results of the experiments again confirmed the theory.[6]

A further extension of the work in the field of highly compressed gases was then made, investigating both theoretically and experimentally the cross effect between chamber geometry as mentioned above and the intermolecular forces.[7] I think this phenomenon can be understood from the following considerations. The constriction causes the flow velocity to be limited by the sound speed. The expansion in the straight portion of the tube, however, is limited by the acoustic impedance of the gas, i.e., the product of the density by the sound speed. Consequently, the co-volume which had no effect in a simple wave type of expansion has an effect when the flow starts from a chamber with a diameter larger than that of the bore.

The co-volume increases the sound speed of the gas. A gas whose equation of state is P(V-b) = RT has a sound speed which is $\frac{V}{V-b}$ times the sound speed of a gas whose equation of state is PV = RT. The acoustic impedance is, however, $\sqrt{\frac{dp}{dv}}$ which is not affected by b (co-volume constant). An experiment was tried. The essence of the experiment was to load air at high pressure into a gun whose chamber was of much larger diameter than the barrel. Only a few shots were made of this type but the velocity obtained from such an air gun was substantially higher than from a gun where the chamber diameter and the bore diameter were equal. Accurate comparison with theory is not yet available.

Section II

I should like now to say something about the work that has been carried on in the field of high temperature. Early in 1949, work was started on the analysis of the effect of inter-atomic forces, that is, the effect of the binding forces of the molecule, on gas dynamic behavior. The theoretical work was primarily concentrated on the attempt to predict the relaxation time of gases that are subjected to a sudden change in temperature as it occurs across shocks. Included in this work was the calculation of the state variables when the air or its constituents, nitrogen and oxygen, is raised to a temperature which is high enough to make the heat capacity of these gases differ from the heat capacity as normally encountered at moderate temperatures. Bethe and Teller had already laid the groundwork for this type of research in their report on shock waves produced by very high pressures. The report is now a classic one.[8] In this work, however, they did not attempt to take into account the properties of the molecules which control the efficiency of transferring energy, which is initially in the translational kinetic energy of the molecules, into an internal vibrational energy.

We therefore undertook to develop a quantitative theory which would permit us to predict the relaxation time of molecules. In doing this, interaction forces between the molecules were taken into account. The theory led to the calculation of the rate at which energy flows from the translational modes into the vibrational modes as a function of the molecular forces, density, and the temperature. Calculations were carried through for oxygen, nitrogen, chlorine, and other diatomic molecules.[9, 10] Experimental check of the temperature dependence was not available.

In 1951, an experiment using the shocktube was conceived and carried through.[11,12] The basic idea of the experiment was to propagate a shock into chlorine. The change in density across the shock was measured by an interferometric method. The behavior of this density would be a discontinuous rise if there were no relaxation times involved. Since, however, the adjustment to the new temperature takes some time, depending on the temperature and the nature of the molecule, a picture such as shown in Figure 1 is observed. The exponential increase of the density (the shift in the fringes) is then directly related to the time it takes for the energy to flow from the translational modes into the internal modes, i.e., the vibration of one atom against the other in the molecule.

Since that time, many others have carried through measurements of this type in oxygen, nitrogen, and carbon dioxide. In general, the theoretically predicted values are quite good.

The theory was then extended to account for the rather remarkable effect of small traces of impurities in the main constituent. A classic example of this is the effect of the presence of traces of hydrogen in chlorine. A hydrogen-chlorine collision is almost two hundred times more effective in exciting chlorine vibration than chlorine-chlorine collision. The theory shows that the basic reason for this effect is that at the same temperature the light molecules such as hydrogen have a much higher velocity. Therefore, the impact of this type of impurity with the main constituent is much shorter duration than the one between the molecules of the main constituent. A little thought shows this to be just as one would expect on the basis of classical concepts. The efficiency of an inelastic collision increases with the shortness of the duration of the impact.

In the course of this work, it became apparent that there were certain impurity effects that did not fall into this category. One of these is the effect of a trace of carbon monoxide on chlorine, or traces of water on most diatomic molecules. A plausible theory to explain this was proposed in 1950.[13,14] The basic idea of this theory is that the near approach of an impurity molecule which has a chemical affinity for the molecule of the main constituent, distorts the internal binding energy of that molecule, weakening it, and permitting an easier transfer of a quantum of energy from the translational to the vibrational mode.

There is an interesting analogy between this effect on the molecular scale and the macroscopic irreversible change in the internal energy of a gas. It is akin to trying to generate a shock by a moving projectile. One way to get an irreversible effect is, of course, to launch the projectile with speed much higher than the sound speed of the gas. However, it is equally possible to cool the gas and produce the same shock with a much lower velocity. Thus if the missile had a cooling effect on the gas ahead of it, then the analogy would be complete. What I am trying to say is that the near approach of the impurity molecule decreases the vibrational frequency of the molecule in the main constituent before it collides with it and transfers the relative kinetic energy into internal vibration.

As early as 1951, it was realized by us that the vibrational excitation would affect, for one thing, the location of the head shock around a blunt missile. It would reduce the shock stand-off distance ; that is, the distance between the missile and the shock. It was further evident that if the relaxation phenomena occurred, then this stand-off distance would become a function of the relaxation time of the gas. A systematic series of experiments was undertaken in which spheres were fired into various gases. The shock stand-off distance was carefully measured. A theory was developed that predicted the stand-off distance as a function of the specific heat of the gas.[15] Later, it was extended to the situation where the change in specific heat varied throughout the region between the shock and the missile itself.

Although the quantitative theory is quite involved, the physical basis is relatively easy to see. The existence of a finite stand-off distance is a consequence of the compressibility of a gas. As the flight Mach number is increased, the gas density between body and shock is increased, the shock coming closer to the body. This increase in density is directly related to the heat capacity of the gas. The density ratio across a shock is given by $\frac{(\gamma + 1)M^2}{(\gamma - 1)M^2 + 2}$ where γ is the ratio of the specific heats, and M the shock Mach number. It is evident that for $M \gg 1$ the density reaches a limit value of $\frac{\gamma + 1}{\gamma - 1}$ times the initial density. For ideal air γ is 7/5; thus the ratio is 6. The shock stand-off distance will reach some asymptotic value. Now, if energy can flow into the vibrational mode of the molecules, the heat capacity is increased and γ decreased to 9/7. The density will rise to a limit value of 8, just as if the compressibility were increased. The shock will come closer to the body to a new asymptotic value. This was confirmed by experiment.[16]

Measurements were extended to the case of finite vibrational relaxation times. This was accomplished by reducing the density of the gas into which the missile is fired, so that the number of collisions among the molecules in the region between the shock and the body becomes so few that there are just not enough collisions for redistribution of the energy between the relative kinetic energy of the molecules and the internal vibrational energy.

The experimental work was then extended to higher missile velocities and hence temperatures, where dissociation,[17] electronic excitation, and ionization take place. Measurements were made to determine the relaxation time of ionization in xenon, where the complexity of a dissociating phenomenon does not occur, and, at the same time, of course, it permits a much higher temperature to be reached for the same missile speeds.

More recently, a theoretical study was made of the radiative heat losses in a shocktube under conditions where the re-absorption of any photon emitted by an excited atom within the gas plays a significant role.[18] Experimental confirmation of this theoretical work is not yet available.

An interesting extension of the shocktube technique to measure the heat conductivity of gases at high temperature was made here in 1955. The problem of measuring the transport properties of a gas at high temperatures is a very severe one. Static measurements are ordinarily made difficult by the high temperatures desired. The shocktube, therefore, is a useful tool in this region of temperature. This technique was used to determine the heat conduction coefficient of argon up to 3000°K.[19]

So much for the work that has been done in the area of high temperatures; the problems of relaxation phenomena, vibrational excitations, dissociation, ionization, and transport properties. I should now like to say a few words about the work that has been done here at the Laboratory in the development of high velocity model launchers, which has, as perhaps a great number of you know, occupied a good deal of our effort.

Section III

Our interest in high-velocity guns took place around 1948-1949 when we undertook to assess the New Mexico School of Mines Hydrogen Gun as a possible military weapon. We made a theoretical analysis of the interior ballistics of this gun and also examined the possibility of making it into a large caliber gun. It was quite evident then that the scheme, although an excellent one for obtaining high velocity and, as a matter of fact is the basis for many of our present high-velocity launchers, was too costly for large-caliber guns. We, therefore, channeled our efforts into accomplishing the same thing by other means. The obvious direction was to attempt to heat hydrogen, not by a single stroke adiabatic compression, but by some chemical reaction. One of the possibilities that we looked into was the use of hydrocarbons, ethylene, and ethylene-acetylene mixtures. The idea there was that these hydrocarbons will decompose when subjected to pressure and then moderately heated. Hot hydrogen and carbon are released. Depending on the temperature after reaction, there is a certain amount of methane formed, too. We experimented with this propellant and did get performances which were better than gunpowder performances but much poorer than that expected from hot hydrogen. There was an interesting relaxation problem involved. When the time for expansion was small, as is the case in the smaller guns, the performance was relatively better than when the length of the barrel was increased, allowing more time for the hydrogen to accelerate the carbon particles and so lose its effectiveness. The effective molecular weight of the propellant was increasing with time.

We then turned to the possibility of using a relatively small amount of hydrogen and oxygen to heat helium or more hydrogen in the chamber. The first work done, in 1953, proved to be quite satisfactory. A 40-mm gun was built that was somewhat larger than the standard 40-mm gun.[20] It was 100 calibers long, the chamber was about twice as large as the standard chamber. Test firings were made at Dahlgren Proving Ground (See Figure 2). A 40-mm sphere, weighing 40 gms, was launched successfully at velocities of the order of 11,500 ft/sec. This was within 5% of the theoretically calculated optimum velocity. Figure 3 is a Fastax movie of the shot. The gun is to the left. In this shot, the gun barrel was not evacuated. The first luminosity seen is from the hot air which was compressed by the sphere in the gun barrel. Then the luminous projectile is seen coming through. It appears as a streak because, although the rate was 12,200 frames per second, the missile moves about six inches in that time. The velocity is clearly seen by referring to the background where the spacing is one foot.

Although the launching of simple models was not too difficult, considerable difficulty was encountered when attempts were made to launch delicate models. The accelerations are enormous. For example, in the 40-mm gun, in order to reach 12,000 ft/sec the projectile weight has to be of the order of 40 gms and the acceleration it is subjected to is nearly 1,000,000 g.

At about this time, the interest in high-velocity guns for shipboard use vanished, because it became obvious that hypervelocity guns would not satisfy the conditions required for shipboard use. They had to be long, they had to have high pressure, they could launch only a negligible amount of payload. As is usually the case, it was difficult to get funds to continue this research since the objective for which the original funding was made had become obsolete. Fortunately for us, interest was then aroused in high-velocity launchers for the laboratory. This removed all of the severe conditions that were imposed for shipboard use. A 4-inch gun was constructed for the 1000-ft. Range. In June 1958, a nylon sphere, four inches in diameter, weighing 1-1/3 pounds (600 gm), was successfully launched. The chamber pressure was only 20,000 psi, about half the design working pressure. The muzzle velocity was 11,300 feet per second, about five percent less than the theoretically expected velocity. Even such velocity was now becoming slow. We had to look for methods of getting higher velocities. Early in 1956 we conceived and designed a new type of gun based on the idea of chemical and shock heating.[21] Figure 4 illustrates the operation. Oxygen-hydrogen reaction heats helium in the front chamber and is then further heated by a shock generated by the second chamber which has steam-heated helium at a higher pressure. Variations on this idea are now used in many laboratories. The main advantage of this type of gun is that for the same bore diameter it is smaller and cheaper than the New Mexico School of Mines single stroke adiabatic compressor type of gun. A 20-mm gun was constructed and launchings of short cylinders were repeatedly made at speeds of 17,500 feet per second. A 2-inch bore diameter gun is now under construction.

Finally, I should like to say something about the fourth area; the research carried on in attempts to produce, control, and determine the nature of high-speed, low-static temperature (high Mach number) gas flows.

Section IV

By 1951, the shocktube had become widely used for flow studies. Its use was also being extended to studies involving high-velocity shocks and high-temperature flows.

Studies in the shocktube were, however, limited to low Mach numbers. In order to be able to do research at high-flow speeds and high Mach numbers, it became obvious that the flow in the shocktube had to be expanded into a larger working section, much as it is done in wind tunnels.

In 1951, we constructed a very small shock tunnel. It consisted of a caliber 0.50 gun barrel and a high pressure chamber. Unfortunately, the expansion of the working gas into a large working section is at the expense of density so that very low densities and pressures were obtained. It was necessary to develop high-pressure, efficient driver gases (propellants). Work was begun on hydrogen-oxygen detonations to drive the working gas in the tube of the shock tunnel. At this time the Cornell Aeronautical Laboratory was also developing such a shock tunnel.

Although the production of high-speed high Mach number flows was thus accomplished, it created a great number of problems in trying to determine the conditions in the working section. This was, in part, due to the very short time that the blow lasts, and partly because of the very low densities that are obtained in the working section at these high Mach numbers. Pressure gages had to be built which were not only sensitive, but had extremely high response times. It was necessary to know as accurately as possible the shock history in the tube, that is, its velocity and the pressure behind it. Fortunately for us the shock intensities were sufficiently high so that the gas behind it was at least partially ionized and would therefore, reflect electromagnetic waves. A technique was then worked out using a microwave interferometer to track the shock front as it moved in the tube. It was possible by this method to get an accurate position-time history of the shock much as was done in tracking pistons in the barrel of a gun.

Around 1955, the Beckman-Whitley High-Speed Framing Camera was developed and was adapted to our uses. With this camera we were able to determine the starting of the flow, its behavior, and its cessation. The difficulties accompanying low densities were still a barrier to the use of this tool for the study of aerodynamic phenomena. It was difficult to be sure that the flow was uniform and steady. Pressure distributions around bodies were difficult to obtain with any accuracy and the measurement of forces on the model was not feasible. It was necessary to develop high-pressure shock tunnels.[22,23]

Soon after the development of our high-speed light gas guns, it became obvious that they would serve equally well as high-pressure shock tunnels. We had only to load the barrel with air. The pressures could be substantially above atmospheric pressures.

Our first shock tunnel, using a 40-mm 200-caliber light gas gun, was put into operation in 1957. Not only pressure distributions and heat-transfer data were beginning to come out, but it was now possible to take schlieren pictures showing characteristic wakes around objects which, until now, were not in evidence on schlieren pictures taken in other shock tunnels. One such picture is shown in Figure 5.

It also became evident that the dynamic pressures in the working section were now sufficient to enable us to measure forces on models even in one to two milliseconds.

A 4-inch light gas gun with a 60-foot long barrel was constructed and installed. It was possible to get millisecond duration blows with this shock tunnel. It now became possible to develop a technique for determining the drag and moments on aerodynamic configurations. This technique will be described in one of the later papers in this symposium. I shall, therefore, say little about it here. The underlying idea consists of suspending light models in the working section of the shock tunnel on fragile suspensions. The start of the blow destroys the suspension leaving the model free-floating in the working section. Because the duration of the blow is so short, the model does not drop any measurable extent. The model is subjected to the accelerating forces of the high-speed flow. The motion of the model is then recorded by the Beckman-Whitley High-Speed Framing Camera which is able to take 82 pictures at a framing rate of over one million. Figure 6 is a picture of this shock tunnel, and Figure 7 is a picture of two cones and a sphere suspended in the flow. The sequence of pictures is approximately 100 microseconds apart and clearly shows the motion of the cones, although in this particular set of pictures, the sphere is not visible.

CONCLUSIONS

In this hurried review of the past ten years' effort I am sure I have overlooked the work of some whose contribution is as significant as those I have mentioned. I hope, however, that I have succeeded in outlining our work in high density gases, high temperature flows, high-speed model launchers, and shock tunnels, and that I have conveyed to you some of the ideas that we have more or less successfully pursued.

REFERENCES

1. Slawsky, Z. I., Michels, A., and Jacobs, S. J. Physica 20, 210 (1954)
2. Michels, A., Slawsky, Z. I., and Jacobs, S. J. Physica 20, 223 (1954)
3. Michels, A., Slawsky, Z. I. and Jacobs, S. J. Physica 20, 1157 (1954)
4. Seigel, A. E., Ph.D. Thesis, Univ. of Amsterdam, 23 Jan 1952 NAVORD Reports 2691, 2692, 2693, and 2694
5. Seigel, A. E., NAVORD Report 3635
6. Seigel, A. E., and Dawson, V. C. D., NAVORD Report 3636
7. Seigel, A. E., NAVORD Report 5707
8. Bethe and Teller
9. Schwartz, R. N., Slawsky, Z. I., and Herzfeld, K. F., J. Chem. Phys. 20, No. 10, 1591 (1952)
10. Schwartz, R. N., and Herzfeld, K. F., J. Chem. Phys. 22, No. 5, 767 (1954)
11. Smiley, E. F., Winkler, E. H., and Slawsky, Z. I., J. Chem. Phys. 20, No. 5, 923 (1952)
12. Slawsky, Z. I., deWette, F. W, and de Groot, S. R., Supplemente al Volume VII, Serie IX, Del Nuovo Chimento (1950)
13. Slawsky, Z. I., de Wette, F. W., and de Groot, S. R., Supplemente al Volume VII, Serie IX, Del Nuovo Chimento (1950)
14. de Wette, F. W., and Slawsky, Z. I., J. Chem. Phys. 22, No. 10, 1701 (1954)
15. Heybey, W. H., NAVORD Report 3594
16. Schwartz, R. N., and Eckerman, J., NAVORD Report 3904
17. Eckerman, J., Ph.D. Thesis, Catholic University of America, May 1958

REFERENCES (Continued)

18. Pomerantz, J., Ph.D. Thesis, University of Maryland, July 1958 (NAVORD Report 6136)

19. Smiley, E. F., Ph.D. Thesis, Catholic University of America, 1957

20. Shepard, B. M., Industrial and Engineering Chemistry, Dec. 1957

21. Seigel, A. E., and Slawsky, Z. I., NAVORD Report 4345 (1956)

22. Slawsky, Z. I., NRL Hypervelocity and Impact Effects Symposium (1957)

23. Seigel, A. E., NRL Hypervelocity and Impact Effects Symposium (1957)

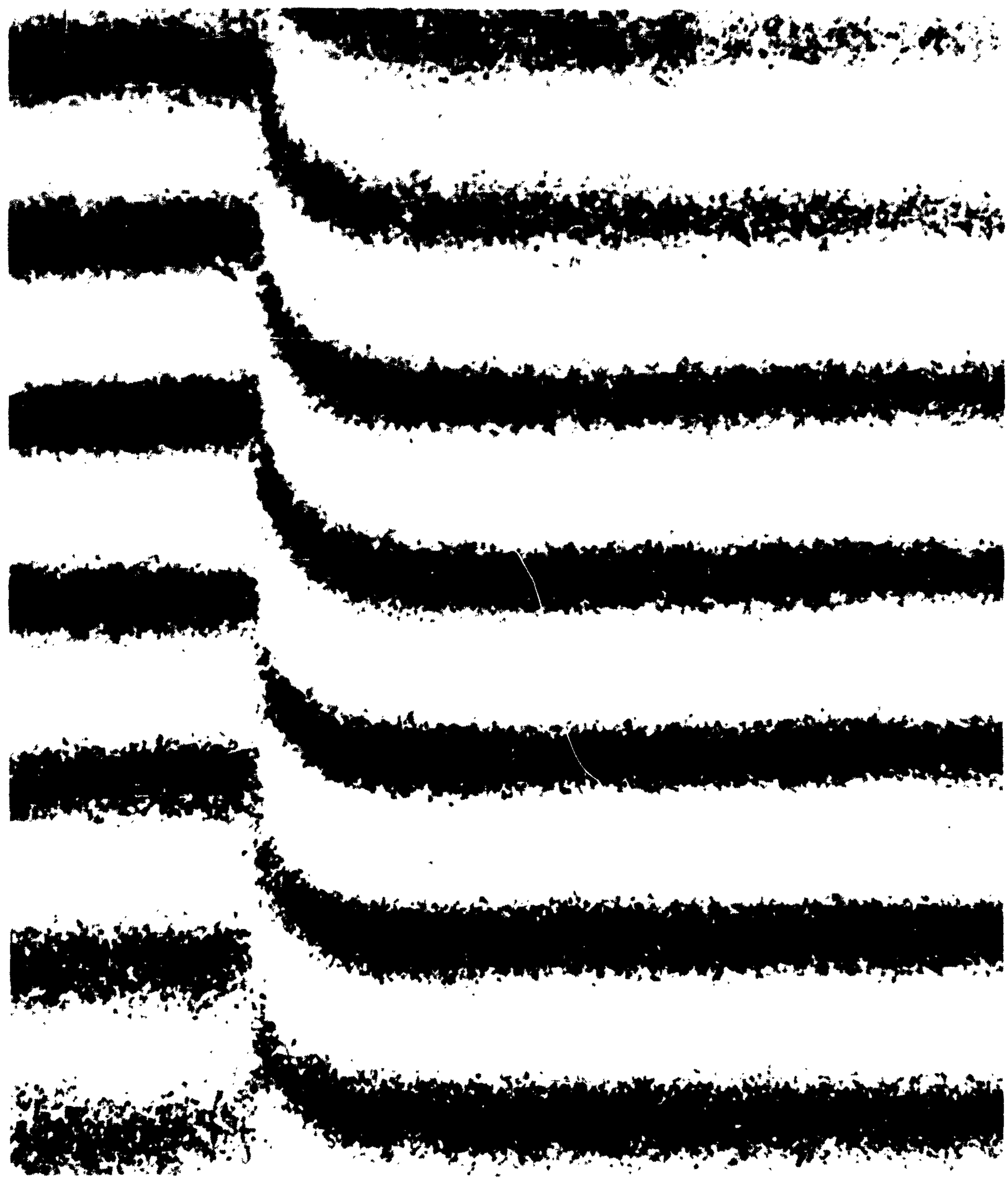


Fig. 1 Interferogram Of Shock In Chlorine

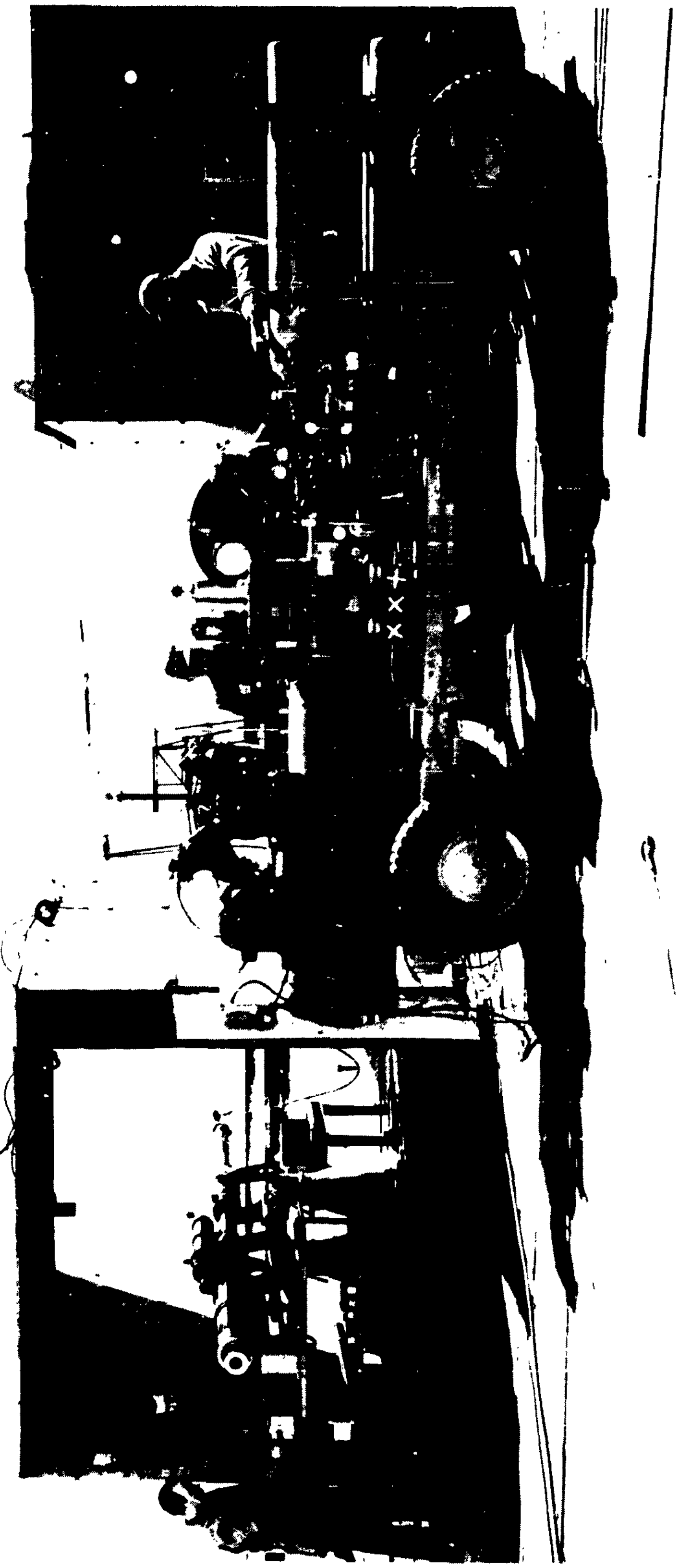


Fig. 2 40-MM Gas Gun At Naval Proving Ground, Dahlgren, Virginia

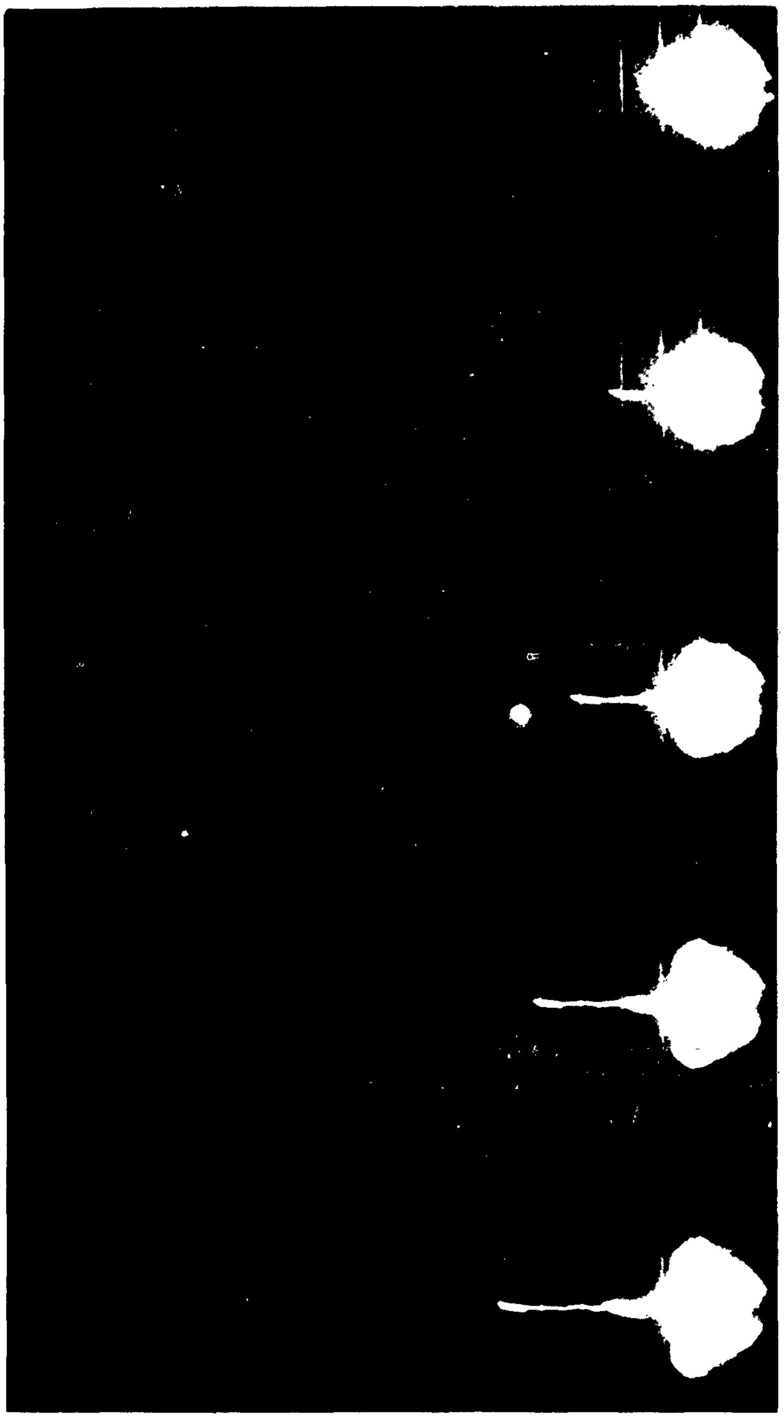


Fig. 3 40-MM Gas Gun - Fastax Movie Of Test Shot

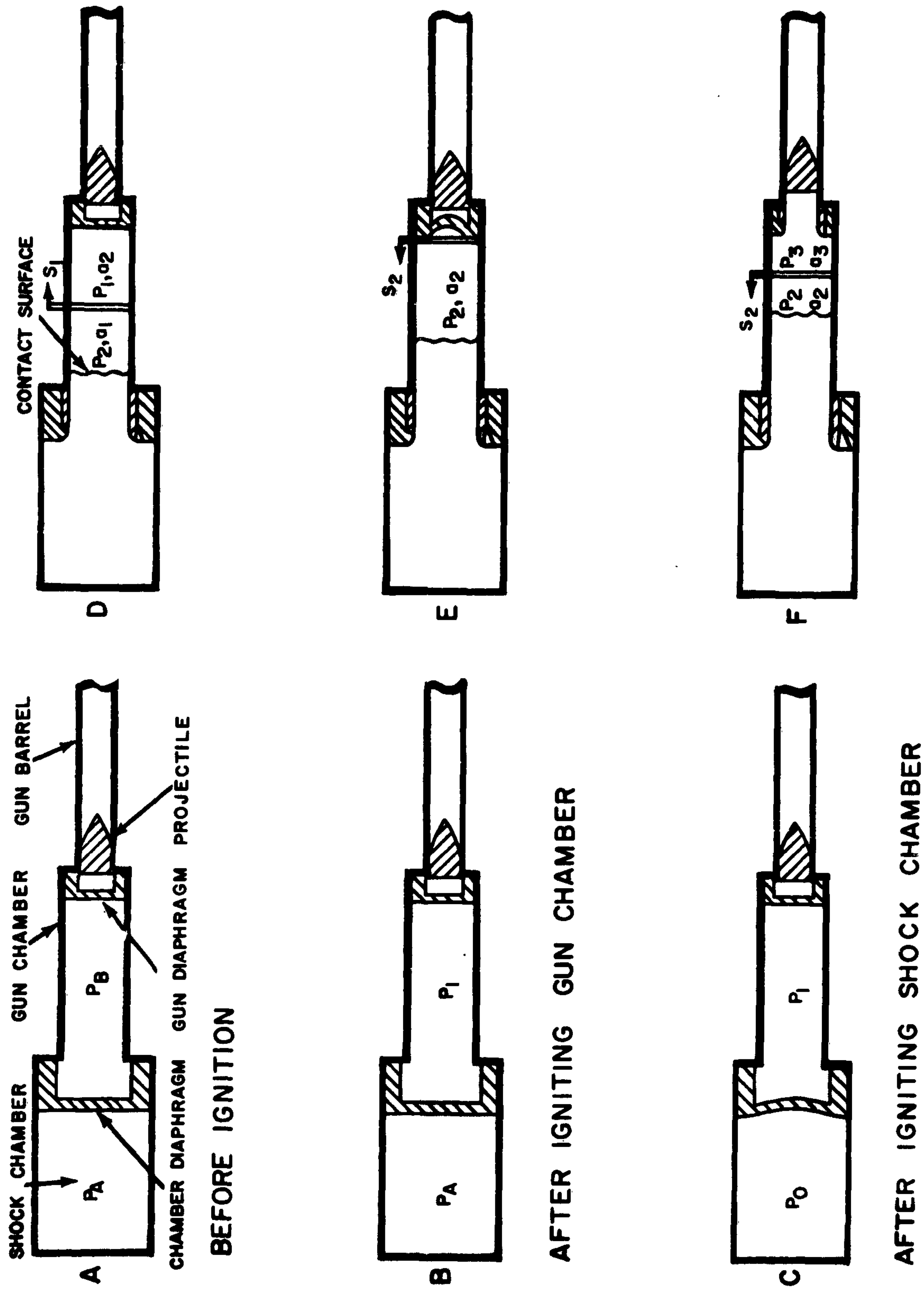


Fig. 4 Schematic Diagram Of Chemical And Shock Heating Principle

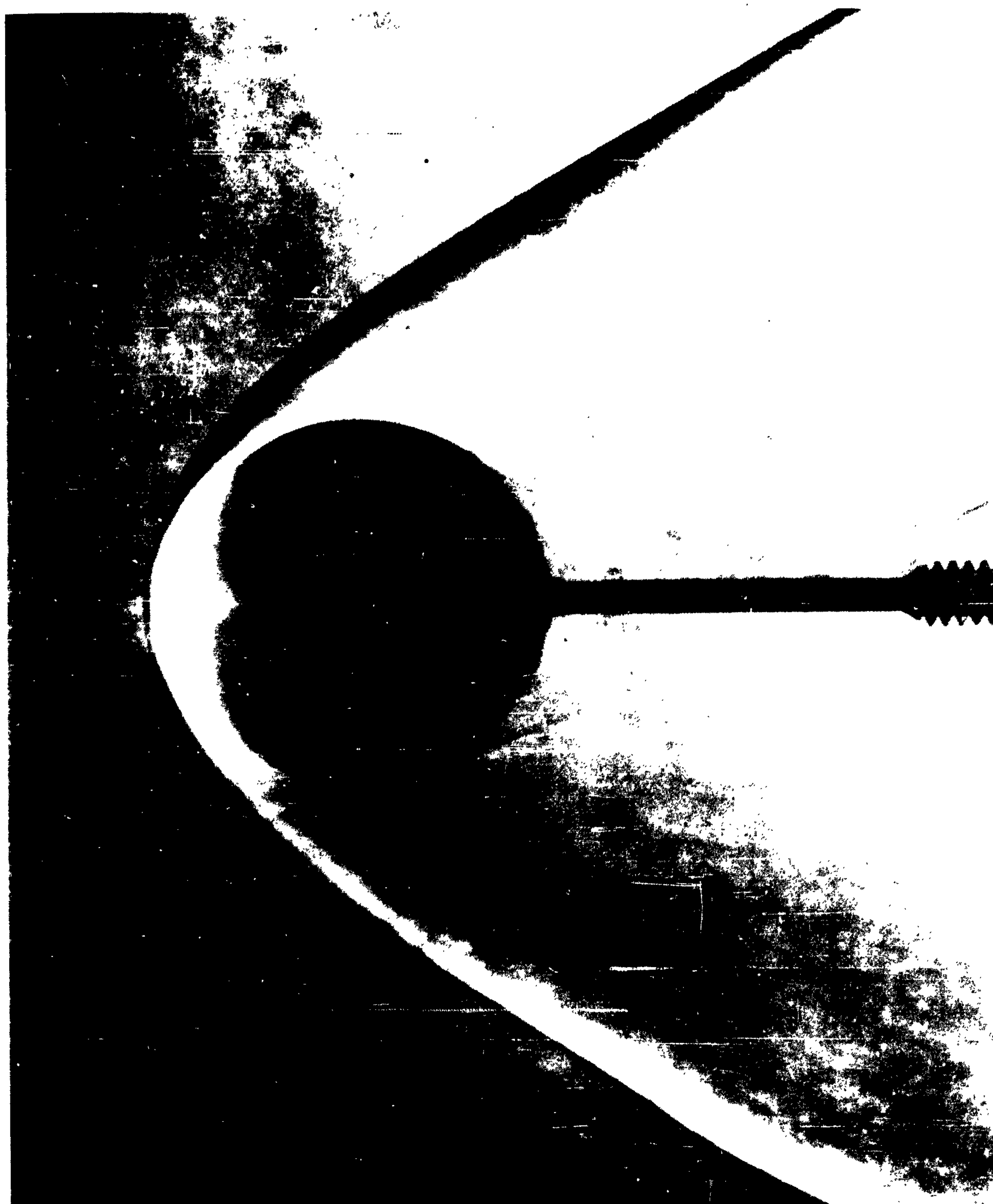


Fig. 5 Schlieren Photograph Showing Characteristic Wake

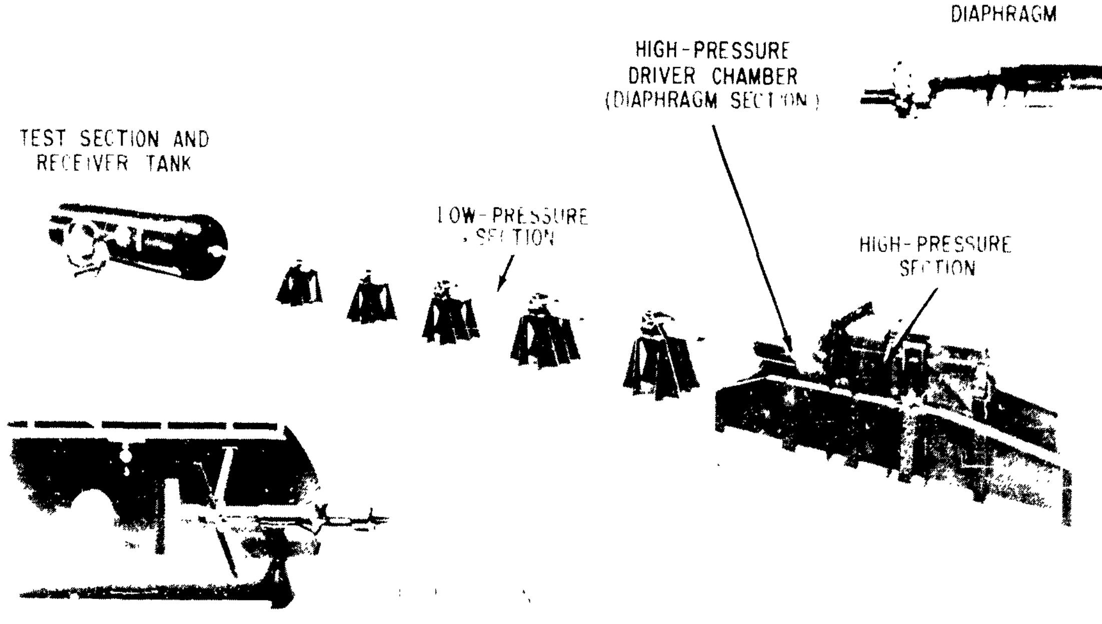


Fig. 6 Hypersonic Shock Tunnel No. 3

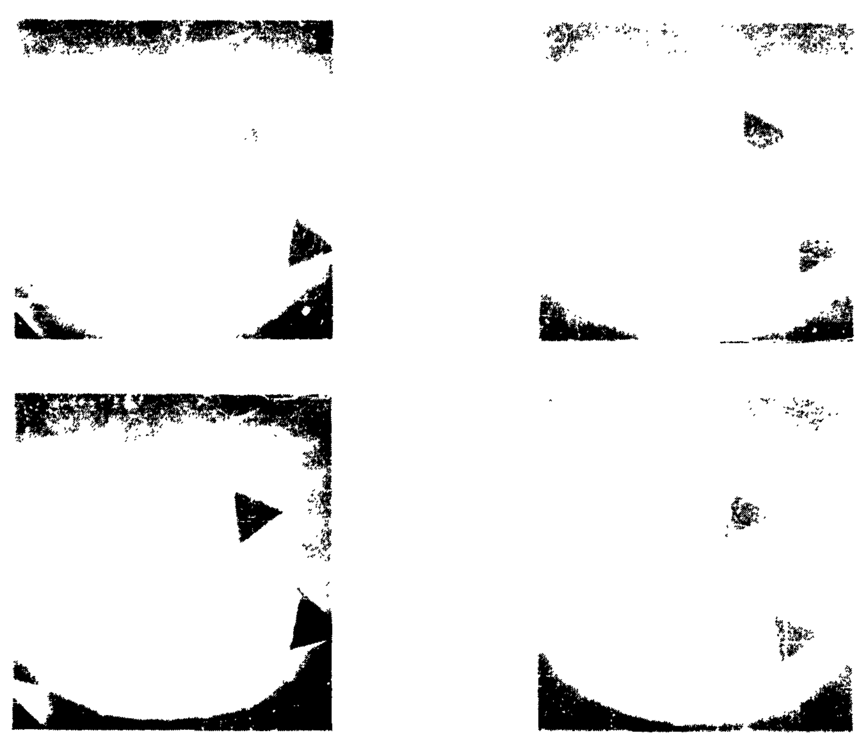


Fig. 7 Sequential Photographs Showing The Motion Of 2 Cones And A Sphere

RECENT ADVANCES IN THE MECHANICS OF HIGHLY RAREFIED GASES

by

G. N. Patterson
Director, Institute of Aerophysics
University of Toronto

INTRODUCTION

I wish to congratulate the Naval Ordnance Laboratory on the occasion of its 10th birthday. In that period, as a consultant, I was privileged to see the rapid growth of NOL from a small group of scientists preoccupied with the design of new facilities to the present modern establishment with a remarkably large output of high quality research. I am sure that the Naval Ordnance Laboratory will continue its major contribution in the future.

The molecular approach to fluid mechanics involves a knowledge of the physical properties of the basic particles of which the gas is composed, the mutual influence of these particles and their action under external forces. The particles can be neutral atoms and molecules or ions and electrons or a mixture. In a neutral gas the characteristics of the motion as a whole are determined by particle-particle and particle-wall collisions. A preponderance of the former leads to a macroscopic (continuum) concept of gas flow and, if the latter only are significant, the motion is essentially microscopic (free molecule flow). The term "collision" applied to an ionized gas has a wider implication, since the motions of the particles of an ionized gas are coupled under the action of long range electromagnetic interactions. Hence in an ionized gas, we must distinguish between long range collective effects and short range particle-particle encounters.

A vehicle launched through the earth's atmosphere meets continually changing physical conditions. The corridor of continuous flight indicates that the speed of the vehicle must be increased with height within specific limits necessary to provide adequate lift and yet avoid excessive skin temperatures. The regimes of fluid mechanics involved are those associated essentially with increasing Mach number and Knudsen number, the stagnation temperature being that corresponding to the Mach number and given atmospheric conditions (Reference 1). The changes with Mach number are those for subsonic, supersonic and hypersonic flow, the latter corresponding to a Mach number above 5 or 6 at which so-called real gas effects first appear, i.e., where temperatures are encountered which produce changes in the particle structure. The ratio of the average distance traversed by molecules between collisions (mean free path, λ) to any characteristic body dimension (ℓ) is called the Knudsen number ($Kn = \lambda/\ell$). This quantity may be made the basis for comparing flow regimes in which the degree of rarefaction is the major parameter. As a gas becomes more rarefied, the Knudsen number increases and the associated rarefaction effects give rise to three essentially different kinds of flows (References 2,3). The first deviation from continuum flow, called slip flow, occurs in the approximate range $0.01 < Kn < 0.1$. When the Knudsen number is very large ($Kn > 10$), free molecule flow occurs in which collisions between gas molecules become insignificant and the transport properties of the flow depend on direct collisions with the vehicle. Transition flow is found in the intermediate range $0.01 < Kn < 10$. This regime is not understood as yet and is presently the subject of theoretical and experimental studies which usually amount to extrapolations from either continuum flow or free molecule flow.

In this paper we are concerned with the mechanics of highly rarefied gases which relate to that region of flow well up in the atmosphere where shock waves and boundary layers have disappeared, and particle-particle collisions are so infrequent that in the absence of electromagnetic effects, disturbance of the atmosphere is produced by direct collisions with the vehicle. In considering this subject we will begin with a gas composed of neutral particles moving at a modest Mach number. Some extrapolation will then be made to include charged particles and high particle energies, where considerable research is now needed.

VELOCITY DISTRIBUTION FUNCTION

A fundamental quantity in free molecule flow is the number of particles striking unit area in unit time. It is interesting to note that a satellite in orbit at 600 miles altitude will encounter about 7×10^{11} particles on one square centimeter of its nose in one second. At 1200 miles altitude the corresponding number is in the neighborhood of 7×10^{7} particles per cm^2 per sec. This impact frequency depends on the distribution of molecular velocities or the velocity distribution function (f).

Let us consider an element of volume $d\tau$ containing a very large number of molecules $n d\tau$. The fundamental question of kinetic theory is: How many of the molecules in $d\tau$ have velocities in a prescribed range $\xi_i, \xi_i + d\xi_i$ ($i = 1,2,3$) at a specific time t? Let us represent the above range as an element of volume in a velocity space, $d\omega = d\xi_1\, d\xi_2\, d\xi_3$.

If we plot the velocities of the $nd\tau$ molecules in the velocity space, they will be scattered over all possible values. The number of points which are plotted in $d\omega$ is the answer to our question above. If f is the density of points in $d\omega$, then the number of molecules in $d\tau$ which have velocities between ξ_i and $\xi_i + d\xi_i$ (that is, in $d\omega$) is $nd\tau \, . \, f \, . \, d\omega$. The symbol f is called the velocity distribution function. In general f depends on ξ_i, x_i, t.

The distribution function is a solution of the Boltzmann equation which takes a simple form in free molecule flow in the absence of external forces if the gas is in steady equilibrium. In this case the Boltzmann equation states that f is constant along a particle trajectory. To determine the distribution function at a given point, we trace the trajectory back to learn whether the particle came from the gas in general or whether it was reflected from the surface of a body. It is evident that some knowledge of molecular reflection from surfaces is needed before the distribution function can be calculated.

MOLECULAR REFLECTION

We can conceive of two limiting kinds of reflection of gas molecules from a solid boundary. If the wall were perfectly smooth, it is possible that "mirror-like" or specular reflection might occur in which the velocity component of the incident molecule normal to the surface is reversed in direction but unchanged in magnitude on contact with the wall. In practice, however, the surface is rough and contains interstices in which a gas molecule may be temporarily trapped. Furthermore, the ultimate direction of reflection may have no relation to the incident direction. This type of reflection will be described as diffuse in character. In diffuse reflection all directions of emission about the normal to the surface are equally probable, regardless of the direction of impingement. More specifically, the probability that a molecule will leave the surface at a particular angle is proportional to the cosine of the angle with respect to the normal. In general the speeds of diffusely reflecting molecules are grouped according to a Maxwellian distribution corresponding to a temperature which can be different from that of the surface.

Gas-surface interactions have been studied mainly by the molecular beam technique. The method is surveyed in References 4, 5. In this method a stream of molecules is directed on a plane surface element and measurements of the flux of scattered molecules are made at various angles relative to the incident beam. The beam is produced by the thermal effusion of molecules from a small source chamber through an orifice or a tube. The molecules emitted by the source move along diverging rectilinear paths. On reaching the orifice the properly orientated molecules pass through and constitute the molecular beam, and those stopped by the orifice are drawn off by a pump. The beam passes through a region of high vacuum (10^{-6}mm. Hg.) and strikes the test surface at a selected angle. The scattered molecules which reflect within a small solid angle pass into a detector and produce a small increment in pressure. The detector (essentially an ionization gauge) can be moved to various positions to determine the complete flux distribution.

Of special interest to designers are the molecular beam tests of air molecules on typical materials used in aircraft construction. Hurlbut (Reference 4) finds that the cosine law of scattering is valid for the spatial flux distribution of air and nitrogen molecules reflected from polished low carbon steel, etched low carbon steel and polished aluminum, independent of outgassing or surface temperature. Furthermore, the reflected molecules possessed a mean energy closely consistent with the thermal condition of the wall.

On the other hand when air and nitrogen molecules reflected from a glass surface, small deviations from the cosine scattering distribution were detected. These deviations could be explained by assuming that while most of the molecules are reflected diffusely, the remainder are reflected specularly. However, experiments using other surfaces and gases show that large deviations from diffuse reflection can occur, and the above method of explaining the difference is in doubt since the large deviation may not be due entirely to pure specular reflection, intermediate types of interaction being quite possible.

Molecular beams of much higher intensities than those effusing from oven sources can be produced by using a small supersonic nozzle (Reference 6). The basic advantage of this technique is in the impartation to heavy molecules of the high velocity of light ones. This process involves a multiplication of energy in the ratio of the molecular weights. Such beams can be more than adequate to meet the requirements of aerodynamic studies of vehicles moving with very high speeds in the upper atmosphere. In fact the energies involved can be of the order of the activation energies for many chemical reactions. Molecular or atomic beams of very high velocity can be produced by the acceleration of ionized molecules through an electric field and then neutralizing them (Reference 7). With these new methods available, further studies of the reflection problem are now possible.

FREE MOLECULE AERODYNAMICS

The subject of free molecule aerodynamics begins with a consideration of the transfer of mass, momentum and energy to and from an element of surface of a body. The fundamental characteristic of free molecule flow is that, on the average, molecular collisions are very infrequent, and the transport process may be regarded as due to incident and reflected streams of molecules which have no mutual interaction. In other words, the transport of mass, momentum, and energy by incoming and emergent molecules can be treated separately.

The free molecule characteristics of the surface element depend on accommodation coefficients which arise from the reflection process. These coefficients are the ratio of the actual resultant flux of momentum or energy to the corresponding resultant flux as it would be if perfectly diffuse reflection occurred. These coefficients have the value one for perfectly diffuse reflection and zero for specular reflection. An actual physical reflection will correspond to accommodation coefficients between 0 and 1. These coefficients require considerably more investigation. Molecular beams available by methods described above should be used for a more detailed study of the reflection process at high molecular energies (Reference 8).

Through the use of appropriate velocity distribution functions (Reference 8) the flux of momentum and energy to and from the surface element can be calculated and the normal and shearing forces and heat transfer obtained. To these results we add the energy balance equation which expresses the equilibrium condition when molecular energy, radiation and externally applied energy are involved. Integration of these forces and energy exchanges around the surface of a given body provides the lift, drag, moments and heat transfer properties of the body (Reference 9). It is interesting to note that lift and drag depend not only on the speed ratio ($S = u/c_m = \sqrt{\gamma/2}\, M$) but also on the ratio of the temperatures of the reflected and incident gases (T_r/T_i). A further result of note is the fact that in free molecule flow the equilibrium temperature of a body can exceed the macroscopic stagnation temperature, a fact readily explained by the kinetic theory of gases (Reference 10).

Increasing interest in minimum drag shapes in free molecule flow has become evident recently. A generalized method of minimizing the drag integral has been applied to the problem of minimum nose drag (Reference 11). This investigation indicates that an optimum axially symmetric nose shape in free molecule flow at high speeds has a flat tip, Figure 1. In the hypersonic extreme the optimum nose shape is somewhat ogival but has a blunt tip and the shape is independent of the ratio of the temperature of the solid surface to that of the undisturbed gas.

A comparison of the limit of free molecule flow (as the speed ratio becomes large) with Newtonian flow can be made on the basis of the pressure coefficient. In Newtonian flow the particles are assumed to move in parallel paths at constant speed. On striking a surface they lose their normal component of momentum. An interesting verification of the pressure coefficient based on the Newtonian theory was obtained in a shock tube during a study of the flow around a hemisphere-cylinder model under test conditions which simulated hypersonic flight, Figure 2. In spite of a complex flow pattern involving a detached bow wave, expansion waves and a boundary layer, the Newtonian pressure coefficient was verified in the supersonic flow regions. The pressure coefficient on a flat plate in limiting free molecule flow depends on the angle of attack, the speed ratio and the ratio of wall and gas temperatures. This pressure coefficient tends to the Newtonian value in the limit provided the temperature ratio remains finite as the speed ratio increases. This condition must be closely scrutinized in the future when very high molecular energies may be involved in orbital flight. Here dissociation, ionization and ablation will be additional factors.

FREE MOLECULE PROBES

One of the most important results of modern research in the field of free molecule flow has been the development of the free molecule probe. The construction of low density wind tunnels has made it possible to obtain flows in which the mean free path is appreciably larger than the probe. At the same time the nozzle and test section are large enough to ensure continuum or slip flow around models. Thus the performance of the probe can be calculated from free molecule theory, and then the probe can be applied to the investigation of more complex flows such as slip flow and shock transition. Such probes have the fundamental advantage that they possess no boundary layer or wave system and they do not sensibly disturb the macroscopic motion of the gas.

The work of developing free molecule probes was initiated at the University of California (References 12, 13). The free molecule aerodynamics of the cylinder was carried a stage further to include the case of the more general velocity distribution function for nonisentropic flows. Thus the properties of a cylindrical wire are known when the probe is placed in a boundary layer or shock wave. The deviation from Maxwellian motion produces additional terms in the expressions for drag coefficient, lift coefficient and the Stanton number which are small compared with the

Maxwellian contribution, Figure 3. The analysis shows that if the speed ratio, energy accommodation coefficient and the velocity distribution function are known for a given flow, then the temperature of the cylinder (or wire) can be calculated.

The free molecule temperature probe was used to determine the internal properties of the shock wave (Reference 14). Investigations of normal shock waves are desirable for various reasons. The one-dimensionality of the flow simplifies the mathematical treatment. No solid boundary is involved as a condition on the transition equations. The deviation from Maxwellian molecular motion depends on a single shock-strength parameter and can be considerable, thus making the properties of nonisentropic flow readily apparent. The deviation is contained in the non-Maxwellian terms in the velocity distribution function which involve the viscous stress and heat flux. In diatomic gases the deviation also arises from the internal degrees of freedom of the molecule.

The profile of the shock wave was measured in terms of the equilibrium temperature of a fine wire embedded in the shock zone and aligned parallel to the plane of the front. According to free molecule theory the temperature of the wire can be calculated if the velocity distribution function is known. In general, when the mean free path is several times the diameter, the stream heats the wire to a temperature which depends on the local speed ratio, static temperature, and the number of excited degrees of freedom of the gas molecules. This is true if the cylindrical wire is a perfect heat conductor internally and is free of radiation and end losses. Free molecule temperature probes can take the form of either a temperature - sensitive resistance wire or a butt-welded thermocouple. The resistance wire responds to an integrated average temperature over the exposed length and is subject to thermal end losses. Thermocouple probes are sensitive only to conditions at the junction. On the other hand, the resistance wire probe can be made in smaller diameters and can be subjected to tension for alignment purposes.

The transition in temperature through the normal shock wave was measured using an equilibrium temperature probe (Reference 14). The experimentally determined transition was compared with the variation of temperature through the shock front calculated on the basis of the Navier-Stokes equations. The comparison was satisfactory if the bulk viscosity was included in the theory.

The shock wave thickness was determined from profiles of this type, and a similar comparison between theory and experiment was made, Figure 4. The thickness is based on the maximum slope of the transition curve and it is plotted in reciprocal form against the Mach number upstream from the shock front.

The measurement of pressure in free molecule flow has been considered in detail at the Institute of Aerophysics (References 15, 16). The orifice probe is a thin-walled tube with a small hole in the side (Reference 17). By rotating the tube relative to the direction of the mass flow the local pressure in any direction can be found, Figure 5. The calculation of the gas pressure from the gauge pressure is based on an equilibrium condition that the number of molecules entering and leaving the hole are the same, that is, no resultant flow of mass occurs. The molecules entering the hole have velocities distributed according to a Maxwellian or non-Maxwellian law (depending on the nature of the gas flow in which the tube is immersed) consistent with the gas temperature and speed rate. The molecules emerging from the opening have velocities distributed according to a Maxwellian distribution consistent with the temperature of the tube.

The pressure-measuring properties of an open-ended tube facing into the mass flow has also been considered. Here we are concerned with the probability that a molecule entering the tube will finally emerge in the gauge volume since some molecules will reflect from the walls in such a way that they are ultimately projected back into the test gas. Similarly some molecules entering the tube from the gauge volume will emerge from the inlet into the test gas while others will finally return to the gauge volume. Again the flux of molecules in each direction depends on the velocity distribution function at the appropriate temperature and speed ratio and the pressure is determined from the condition that the resultant flow of mass between the gas and the gauge volume is zero. The long-tube impact probe has been investigated theoretically and experimentally (Reference 15). The results are shown in Figure 6. Over the range of speed ratios for which the theory applies, the agreement is good. The properties of these probes at high speed ratios await further study.

The orifice-type free molecule probe has been applied to the investigation of the low density flow around a flat plate. In this application the pressure properties of the probe must be recalculated to include the effect of the plate. Molecules entering the orifice now come from the gas flow and from the plate by diffuse reflection. The two streams of molecules are independent and their effect on pressure can be determined by using the distribution functions appropriate to the temperatures of the gas and plate. Near the leading edge of the plate these distributions will be Maxwellian (Reference 18). The calculated pressure at various distances from the plate has been substantiated if allowance is made for flow gradients (Reference 18).

Experiment shows the existence of a region of free molecule flow at the leading edge of this plate, Figure 7.

Another device which can be used to examine the properties of a flow at low density without appreciable disturbances of the macroscopic motion is the electron gun (Reference 19). This probing instrument can determine the density at a point in a flow of any dimensionality. A photomultiplier scanning system is used to measure the illumination along an electron beam which traverses the low density flow. Beams about a foot in length without appreciable attenuation have been obtained in the UTIA low density tunnel under free-molecule and transition conditions of flow. Many problems in rarefied gas flows now await the application of this device. In the future the electron gun may be adapted to the measurement of local velocity, Figure 8, and ultimately to the spectroscopic determination of the composition of plasmas.

FLOW OF IONIZED GASES AT LOW DENSITY

In the free molecule flow of a neutral gas, when no external forces are acting, the molecules move in straight lines until they collide with a surface. In an ionized gas flowing in the presence of a magnetic field, a higher degree of generality is introduced. The ions move in special paths around the lines of force on axes, the circular component of the motion being induced by the magnetic field. An electron moving in the earth's magnetic field may make a million revolutions in a second while an ionized atom may complete the circular motion one or two hundred times a second. These numbers are substantially larger under laboratory test conditions. At ordinary pressures collisions occur so frequently that an ion travels along a free path which is approximately linear and the only effect of the magnetic field is to deflect the current-carrying electric charges. At very low pressures the situation is substantially different. The ion makes many turns before a collision occurs and therefore spirals freely. On the other hand, the magnetic field opposes the movement of ions perpendicular to the lines of force in the direction of an applied electric field. Thus the effect of the magnetic field is quite different at high and low pressures.

A particle collision takes on a new significance in an ionized gas. In addition to the short-range particle-particle encounters we also have long-range electromagnetic interactions. The relation of the collision time for particle-particle collisions to the characteristic period or transit time of the macroscopic motions is important. If the collision time is larger than the periods of the flow, then the particle encounters may be neglected and we have the case of the motion of an ionized gas at low density. However, the long-range electromagnetic effects remain and they have a considerable effect on the particle motions as described above. Basically these effects are apparent in the modified form of the Boltzmann equation for the distribution of particle velocities in a collision-free flow. Here is a field of research with great interest and possibilities. It leads on to the study of high temperature plasmas where properties can be described by the collision-free form of the Boltzmann equation.

REFERENCES

1. Patterson, G. N.: Molecular Approach to Problems of High-Altitude, High-Speed Flight, UTIA Review No. 10

2. Tsien, H. S.: Superaerodynamics, Mechanics of Rarefied Gases, J. Aeronaut. Sci. Vol. 13, p. 653 (1946)

3. Schaaf, S. A.: Rarefied Gas Dynamics, Applied Mechanics Reviews, Vol. 9, No. 10, p. 413, 1956

4. Hurlbut, F. C.: An Experimental Molecular Beam Investigation of the Scattering of Molecules from Surfaces, Report No. HE 150-118, Institute of Engineering Research, University of California, 1953

5. Smith, K. F.: Molecular Beams, Methuen & Co., 1955

6. Kantrowitz, A., Gray, G.: High Intensity Source for the Molecular Beam, Review of Scientific Instruments 22, 328, 1951

7. Amdur, I., Pearlman, H.: High Velocity Atomic Beams, J. Chemical Physics, 8, 7, 1940

8. Patterson, G. N.: Molecular Flow of Gases, John Wiley & Sons, New York, 1956

9. Stalder, J. R., Goodwin, G., Creager, M. O.: A Comparison of Theory and Experiment for High-Speed, Free-Molecule Flow, NACA Report No. 1032, 1951

10. Stalder, J. R., Goodwin, G., Creager, M. O.: Heat Transfer to Bodies in a High-Speed Rarefied-Gas Stream, NACA Report No. 1093 1952

11. Tan, H. S.: Nose Drag in Free-Molecule Flow and its Minimization, Therm. Inc., Report No. TAR-TR No. 1, 1959

12. Patterson, G. N.: Aerophysical Problems of Flight at Extreme Altitudes and Speeds, Can. Aero. J., Vol. 4, Nos. 1, 2, and 3, 1958

13. Bell, S., Schaaf, S. A.: Heat Transfer to a Cylinder for the Free Molecule Flow of a Nonuniform Gas, Jet Propulsion, April, 1955

REFERENCES (Continued)

14. Sherman, F. S.: A Low-Density Wind-Tunnel Study of Shock Wave Structure and Relaxation Phenomena in Gases, NACA Tech Note 3298, 1951

15. Harris, E. L., Patterson, G. N.: Properties of Impact Probes in Free Molecule Flow, Institute of Aerophysics, University of Toronto, Rep. No. 52, 1958

16. Enkenhus, K. R., Harris, E. L., Patterson, G. N.: Pressure Probes in Free Molecule Flow (to be published)

17. Enkenhus, K. R.: Pressure Probes at Very Low Density, Institute of Aerophysics, University of Toronto, Rep. No. 43, 1957

18. Harris, E. L.: Investigation of Free-Molecule and Transition Flows near the Leading Edge of a Flat Plate, Institute of Aerophysics, University of Toronto, Rep. No. 53, 1958

19. Schumacher, B. W., Gadamer, E. O.: Electron Beam Fluorescence Probe for Measuring the Local Gas Density in a Wide Field of Observation, Canadian J. of Phys., Vol. 36, p. 659, 1958

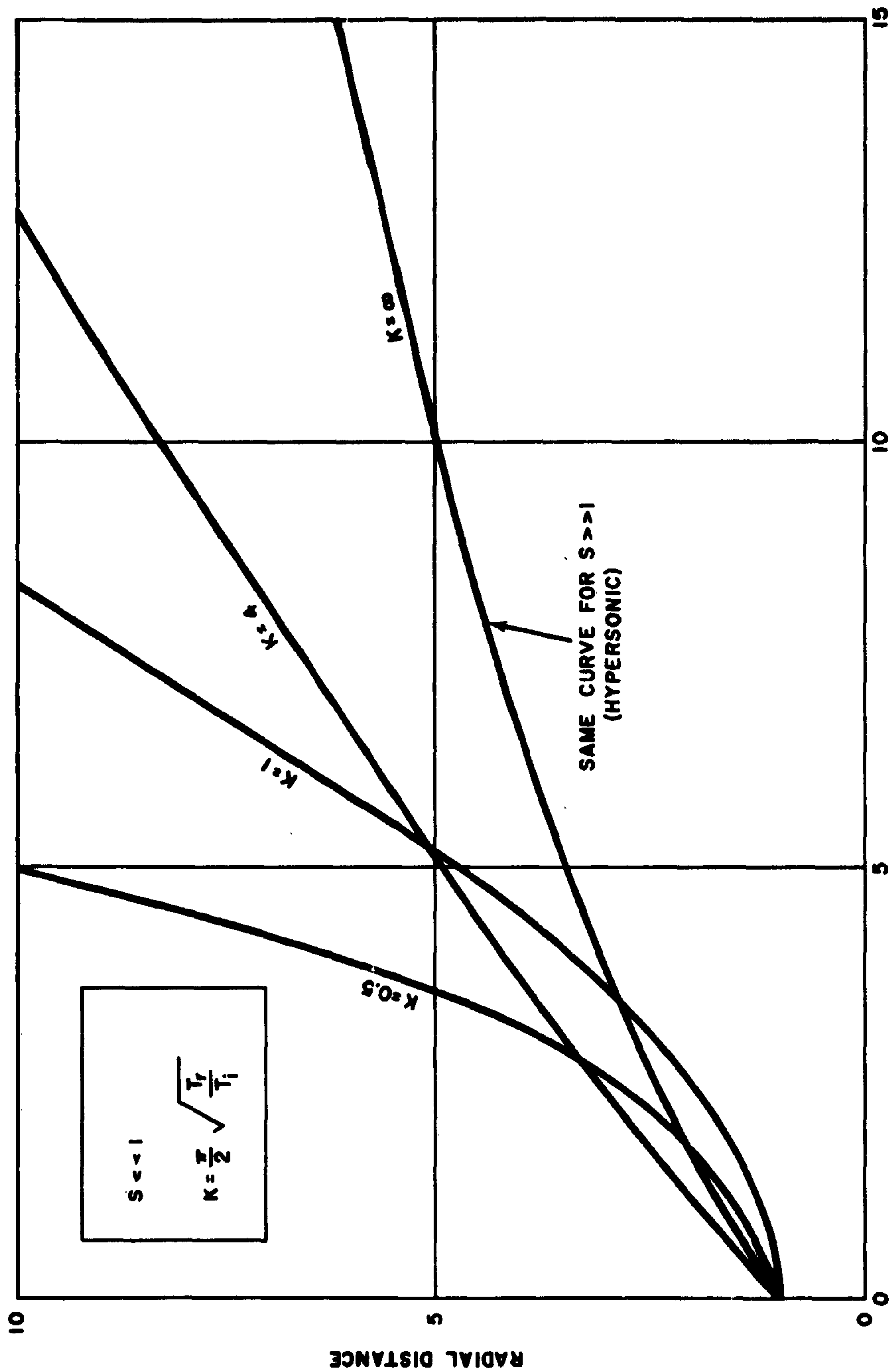


Fig. 1 Nose Shapes For Minimum Drag In Free Molecule Flow

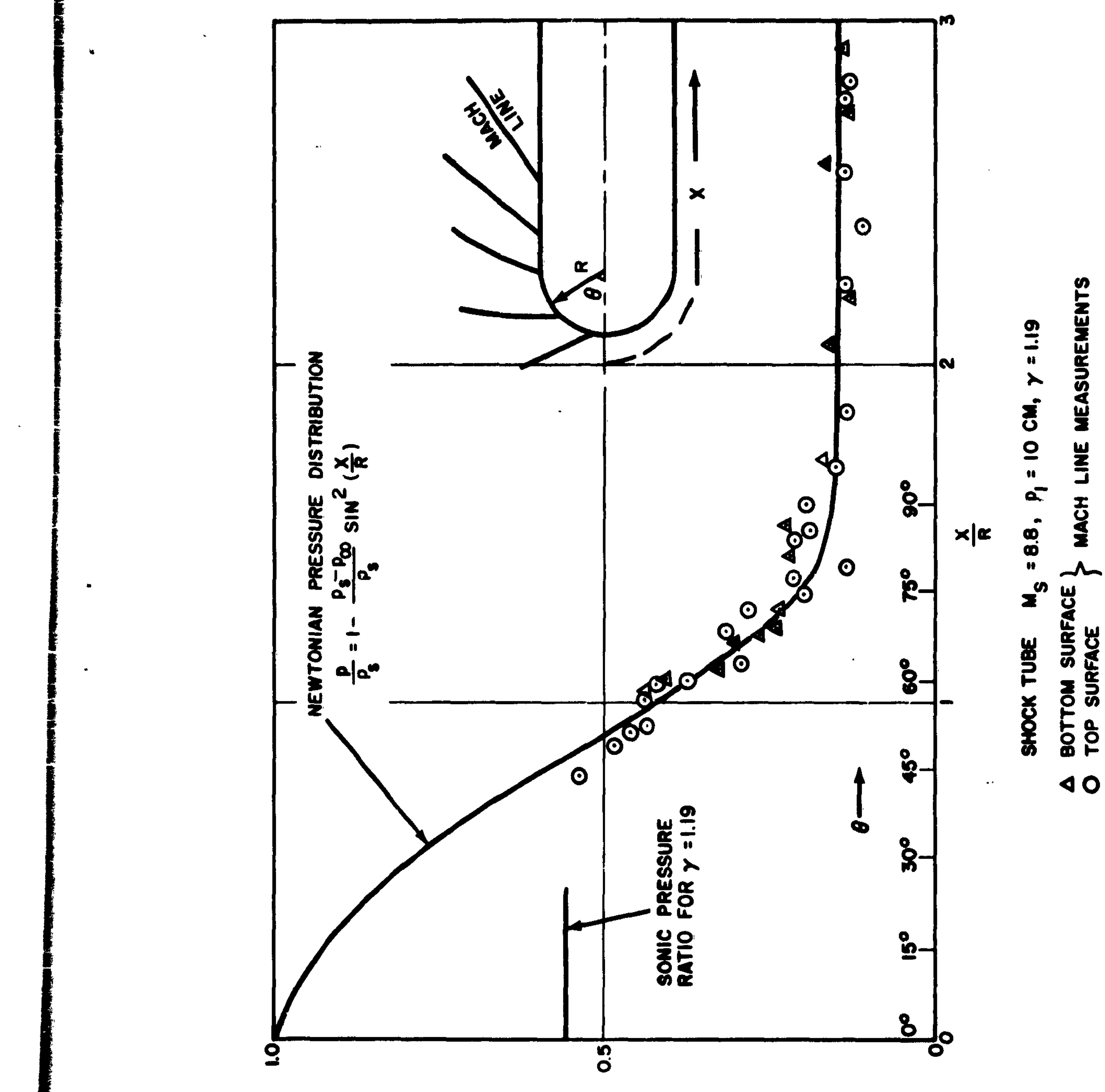


Fig. 2 Pressure Distribution In The Region Of Supersonic Flow On The Surface Of A Blunt Body At High Mach Number

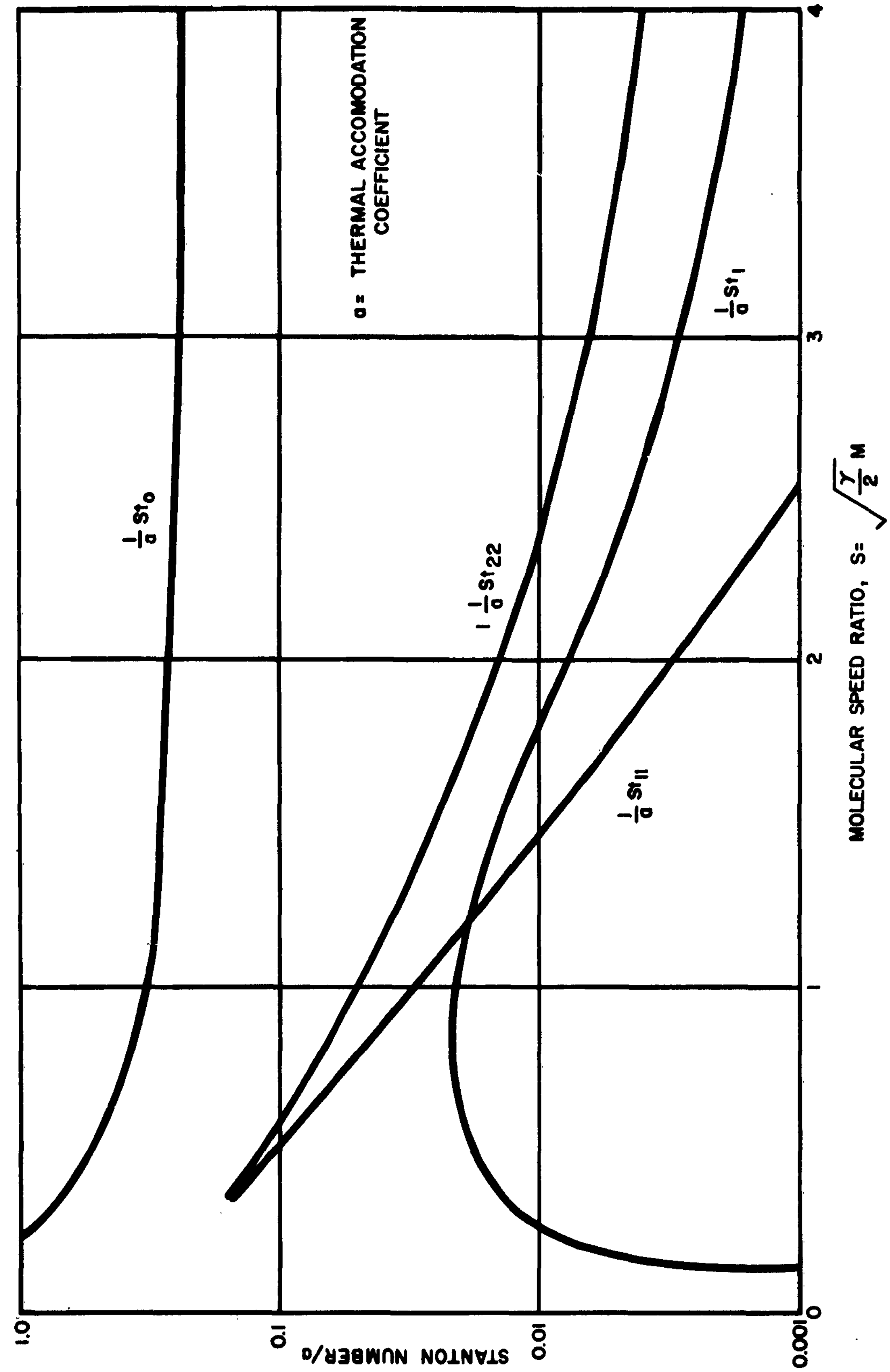


Fig. 3 Heat Transfer Properties Of A Cylinder In Non-Isentropic, Free-Molecule Probe

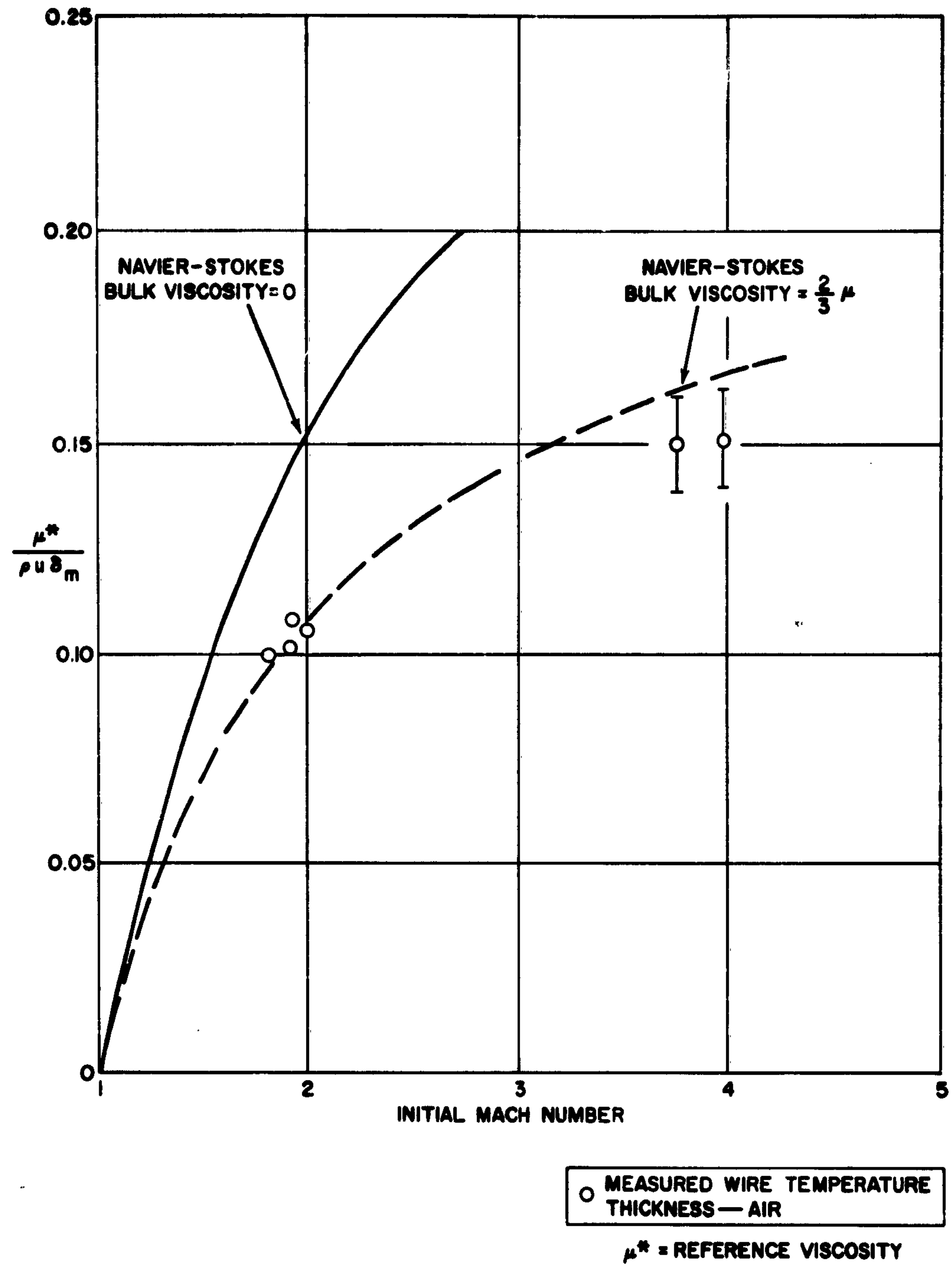


Fig. 4 Measurements Of Shock Thickness With An Equilibrium Temperature, Free-Molecule Probe

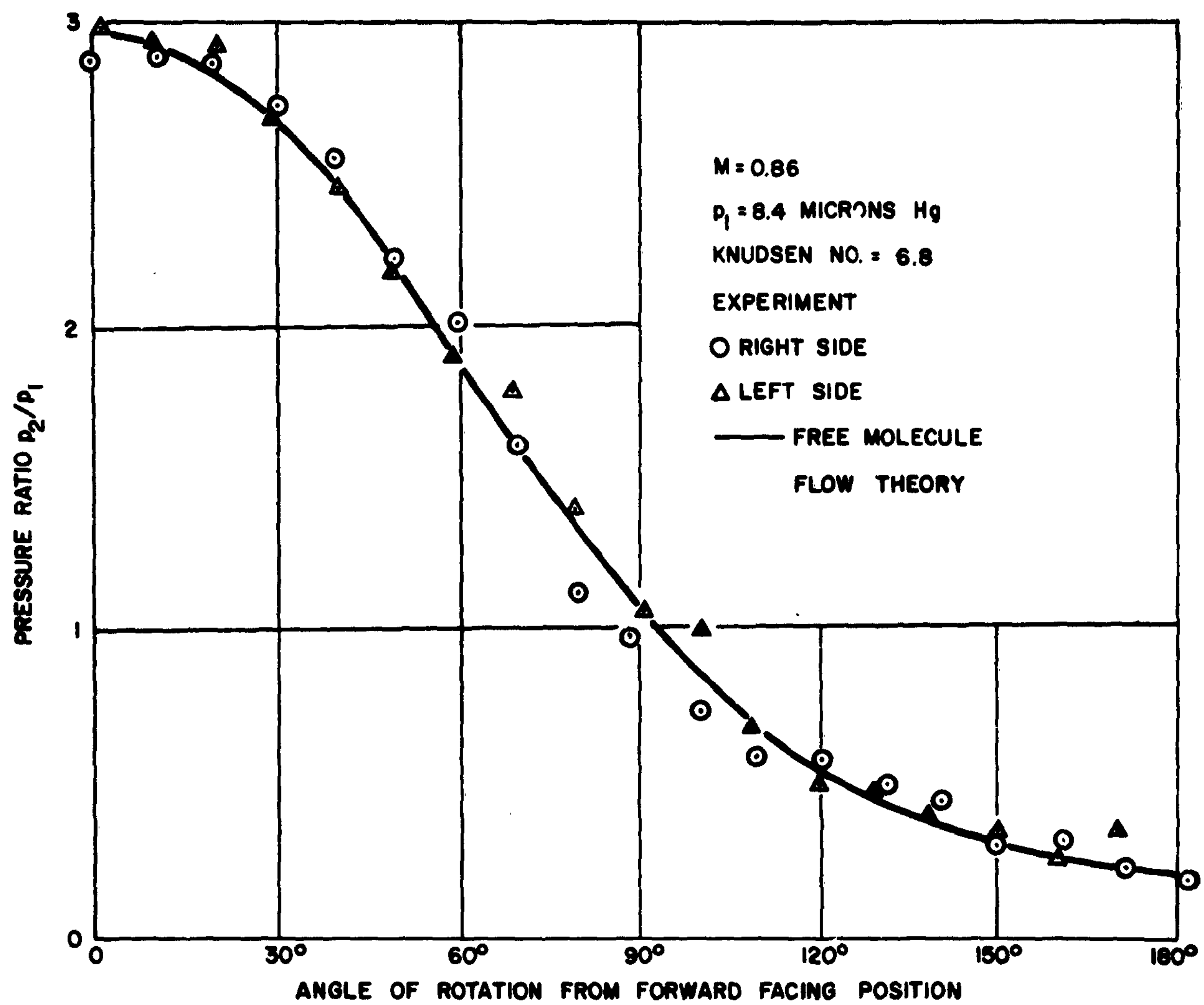


Fig. 5 Orifice Pressure Probe In Free-Molecule Flow

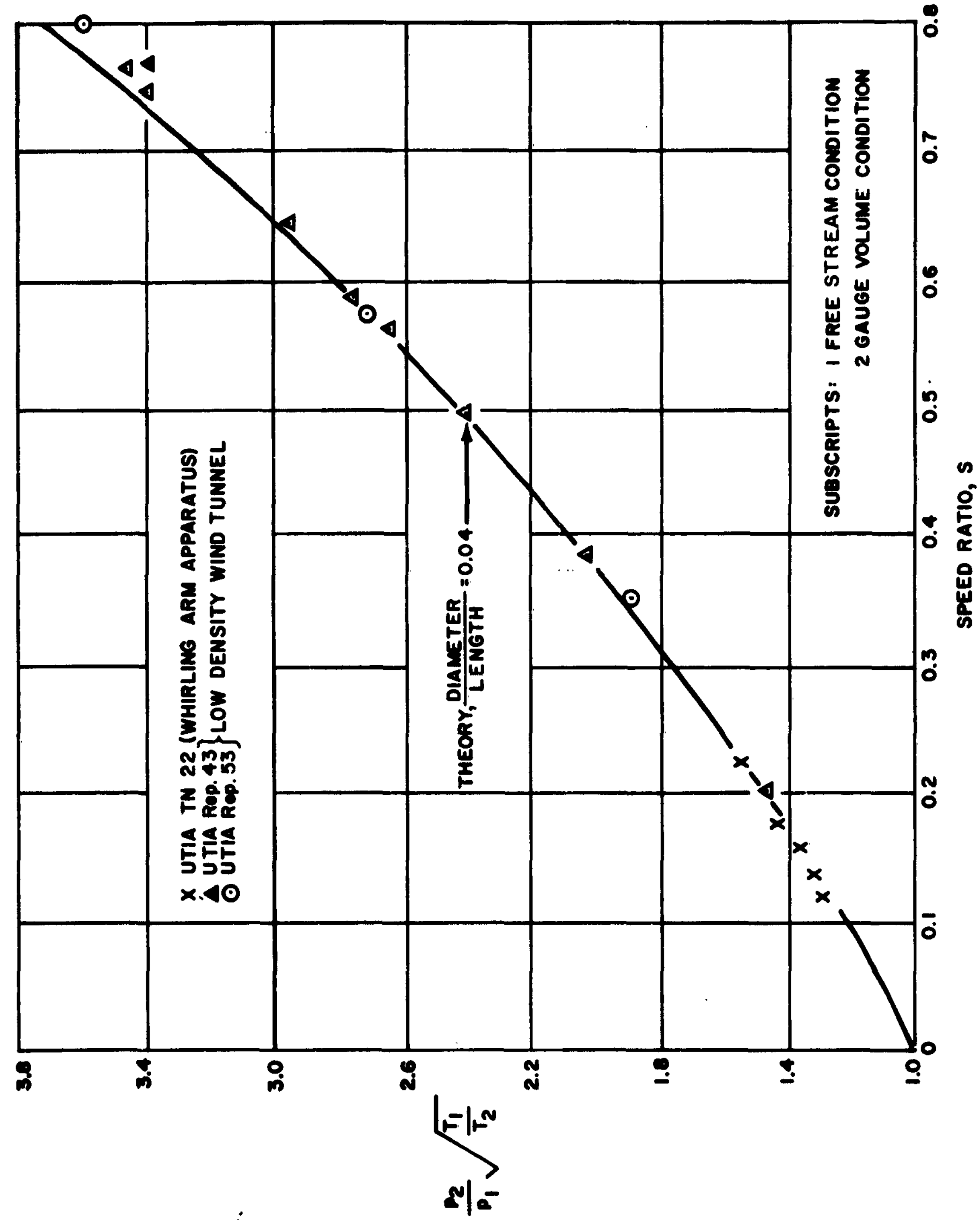


Fig. 6 Impact Pressure Probe In Free-Molecule Flow

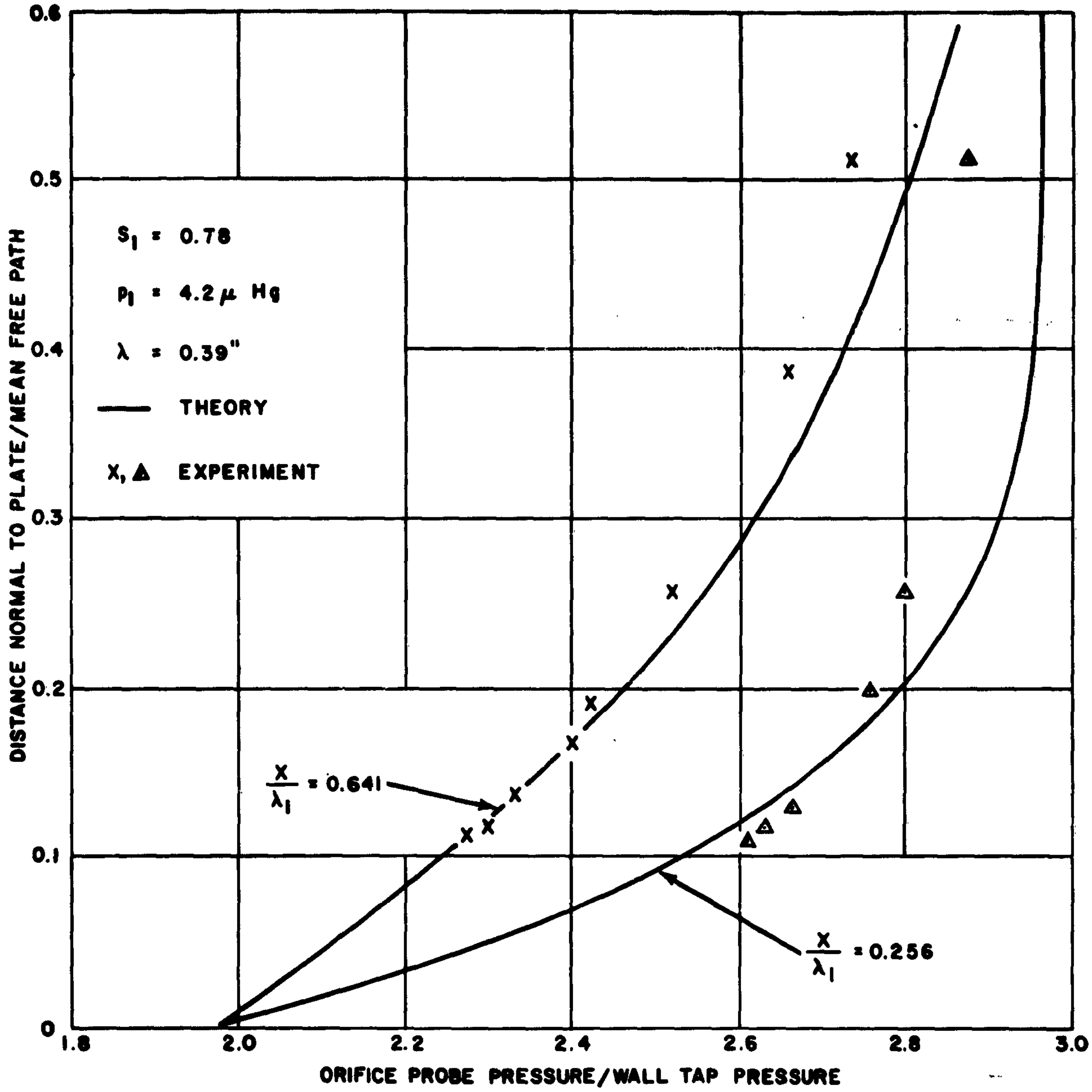


Fig. 7 Pressure Distributions Near The Leading Edge Of A Flate Plate At Very Low Density

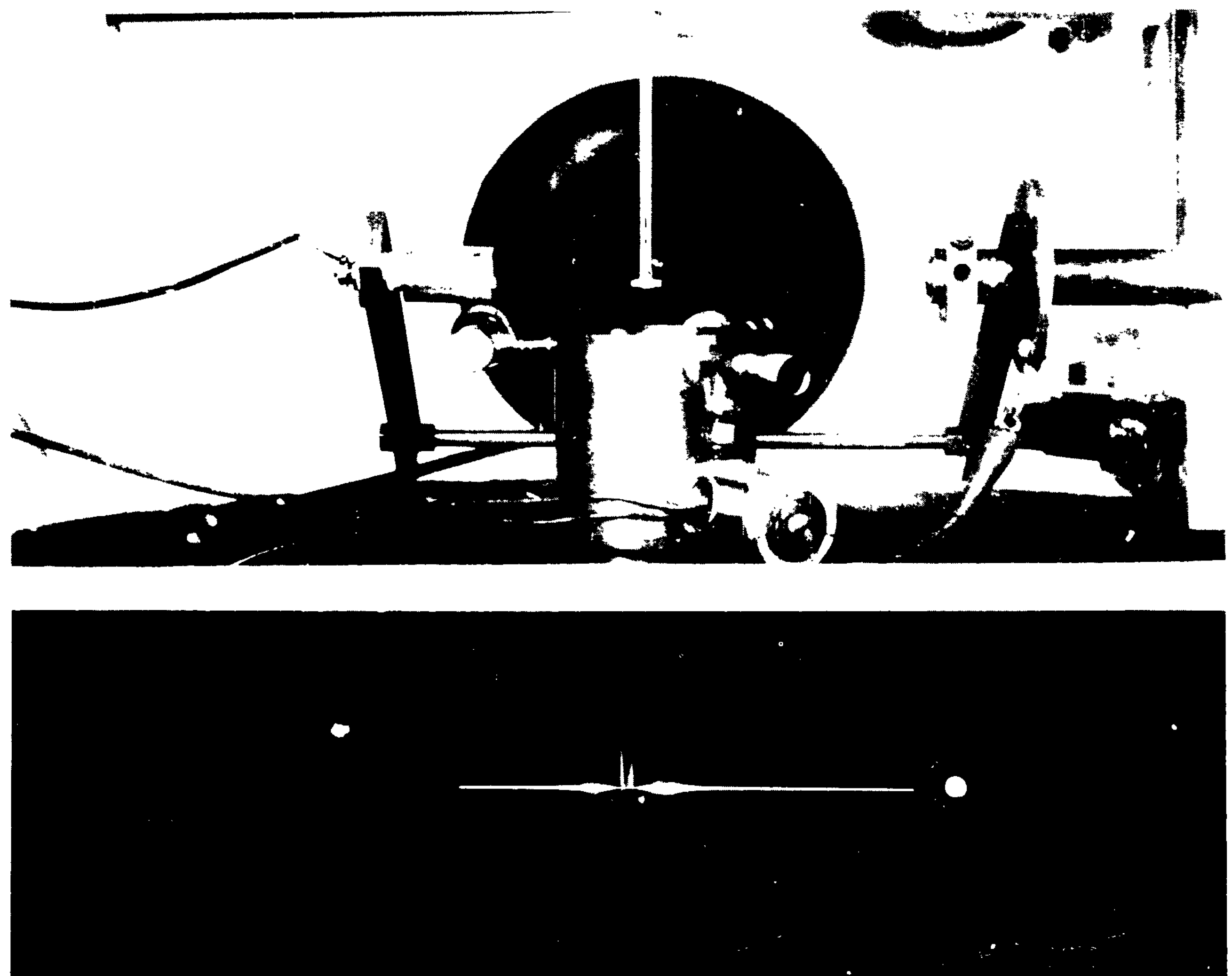


Fig. 8 Application Of The Electron Gun For The Point Measurement Of Density

SURVEY OF NOL APPLIED MATHEMATIC RESEARCH

by

Dr. Richard C. Roberts
Chief, Mathematics Department
U. S. Naval Ordnance Laboratory

Introduction

This paper is concerned with the activities of the Mathematics Department for the last ten years. During this time, the Department has undergone very great changes. These changes have been brought about primarily by the impact which high-speed computing has made on the research and development effort in the Naval Ordnance Laboratory. For example, in 1949 the bulk of the research effort in the Department was supported by funds from the Office of Naval Research and NOL Foundational Research. Now we are receiving most of our support from project funds which come either directly from BuOrd or from other Departments in the Laboratory.

Research Activity

From the beginning of mathematical activity at NOL there has been great interest in various aspects of the study of partial differential equations. Stimulated by von Neumann, important work on the stability of finite difference solutions of partial differential equations was carried out here. This was done at a time when the pitfalls associated with using unstable finite difference schemes were not generally recognized. A picture of what is involved may be gained by looking at the heat equation. For the simplest finite difference scheme, which represents the differential equation of one dimensional heat conduction, one thing stands out. If the ratio of the time increment divided by the square of the space increment is taken larger than a certain critical value, unstable oscillations occur. If the ratio is less than this same critical value, then the solution behaves in a reasonable manner. These phenomena are shown in Figure 1. This work has received quite widespread recognition. The paper that was published giving the details of the investigation is nearly always referenced in any work concerned with this type of stability.1)

We turn now to the general topic of the solution of hyperbolic partial differential equations. Our effort recently has been centered on the study of certain important types of non-linear hyperbolic equations. An example of a general type is shown in Figure 2. This equation has two independent variables. Three different types of initial or boundary data are indicated. The first are Cauchy data where the dependent variable and its normal derivative are given on a noncharacteristic curve. The second are characteristic data where the dependent variable is given along two characteristic curves, which in this case are the coordinate curves themselves. In the third case we have mixed data where the dependent variable is prescribed along both a characteristic and noncharacteristic curve. Several interesting theorems relating to the existence of the solutions to the three problems mentioned above have been found. The proofs are of a constructive nature which lead to methods by which a numerical solution can be obtained.[2,3] Very recently some work has also been completed on an analog of the well-known Runge-Kutta method for solving ordinary differential equations applied to partial differential equations of this type.[4]

Another aspect of the solution of hyperbolic partial differential equations has occupied us for a number of years. This has been the numerical solution of compressible flow problems by the method of characteristics. We have been quite successful in applying this method to a number of interesting problems. An application of the method to nozzle design will be presented elsewhere in the program.[5]

The investigation of shock waves has occupied the Mathematics Department for many years. Work has been done on various aspects of the theory of shocks in a compressible medium. This has included the reflection, refraction and interactions of shocks.[6] By way of example, Figure 3 shows a plane shock incident on a corner. Both the incident and reflected shock may be seen. The constant pressure curves for this case are shown in Figure 4.

The next topic is an important one and will be covered in a little more detail. This is a basic study of Magnus force. If a spinning cylinder is placed in a cross flow it has been known for many years that you get a side force which is a function of the rate of spin. Figure 5 gives a pictorial representation of this situation, while Figure 6 shows some of the early experimental data. Figure 6 is a plot of force coefficient versus the ratio of the cylinders peripheral velocity to the free-stream velocity of the flow. It is seen that the force gradually rises as the angular velocity increases and finally becomes essentially constant when the velocity ratio is sufficiently large. These early measurements indicated that zero force occurred at a small positive value of the velocity ratio. Only rather recently has an explanation of this phenomenon been given. The theoretical work was done here at NOL. It indicated that for a certain range of low speed ratios, the boundary layer on the top of the cylinder was laminar while on the bottom it was turbulent, as shown in Figure 7. Within this speed ratio regime the Magnus force is actually negative. As the angular velocity is increased the boundary layer eventually becomes completely turbulent so that the Magnus force passes through zero and becomes positive. It is also zero for zero velocity ratio. This behavior is, of course, dependent on the Reynolds number. In Figure 8 the hatched area indicates the region of combinations of Reynolds number and velocity ratio at which the Magnus force will be negative.7) Figure 9 is a comparison which was made with recent experimental data. Theory and experiment appear to be in reasonable agreement.

As has been indicated in the paper given by Dr. Lobb, the Mathematics Department has cooperated quite closely with the Aerodynamics Department, particularly in the field of boundary layer computation.8) In addition to the work already discussed, other types of boundary layer investigation have been made. In particular, a computer code has been devised that handles the boundary layer and heat conduction on a body re-entering the earth's atmosphere. This includes the simultaneous treatment of laminar, transition and turbulent regions of the boundary layer when they occur on the body under consideration. The boundary layer and heat conduction phases of the program just mentioned may be split apart and used separately if so desired.

Some other very interesting problems are being attacked in cooperation with the Explosions Research Department at NOL. These come under the general title of explosive hydrodynamics. Figure 10 illustrates a simple example of such a problem. Given that a plane detonation wave propagates down a stick of explosive capped with a piece of metal, one would like to know how the metal behaves after being hit by the detonation, e.g., whether it might spall or split apart.9) The von Neumann-Richtmeyer method has been used in solving this problem. This is a uniform finite difference approach using an artificial viscosity tern that allows shocks to appear automatically. At the high pressures occurring in explosive processes, the assumption is usually made that any solid elements in contact or close to the explosive can be considered to behave like a fluid. This enables the same types of equations to be used in all parts of the problem. At the present time, this scheme is being applied to some very complicated problems where explosive material is interspersed with other inert materials.

Turning now to numerical analysis, there are two pieces of research which should be mentioned. The first has to do with the subject of the acceleration of convergence of sequences by means of certain non-linear transform techniques.10) For example, given a sequence of numbers A_n, a new sequence B_n is defined as shown in Figures 11 and 12. The symbol delta occurring in Figure 12 indicates the usual first forward difference. By means of the Shanks' transform, a slowly convergent sequence (A_n) can be replaced by a sequence (B_n) which is usually much more rapidly convergent. By way of example, consider the well-known Leibnitz series for pi, shown in Figure 13. The first column gives the first few partial sums and indicates how slowly the series converges. The subsequent columns indicate the result of applying the first-order form of the transform once, twice, and three times respectively. Thus, while it would take over one million terms to get five or six place accuracy, Figure 13 indicates that such accuracy has already been reached in the last column. This transform technique has been invaluable in many problems here at the Laboratory.

The second item in numerical analysis is that of curve fitting. Good curve fitting is almost an art rather than a science. An example of what some experts can do is shown in Figure 14. In this figure, the drag coefficient for spheres is plotted as a function of Mach number. Everyone has seen data of this type and knows that its primary feature is the rapid rise in drag coefficient at Mach number one. Fitting data like this with a single analytic expression is quite a feat. The analytic expression used was a rational function type of approximation and fits the data very closely.11) Similar techniques have been applied to other problems.

The last piece of research to be mentioned here is that of automatic programming. A few years ago, a coding system was designed here known as ADES.12) The system was checked out on the IBM 650. The 650 tests showed that ADES could be made to work but that a computer with tape was needed to make it completely feasible. Since NOL has gotten its 704, it is hoped that ADES may be recoded for it. Some of the features of ADES are felt to be superior to such existing systems as FORTRAN.

Service and Support

The types of service which the Mathematics Department gives to other areas of the Laboratory are for the most part concerned with consultation in various mathematical fields, programming of problems for the IBM 704, educating all interested people in how to code for our computer, and providing design help and guidance in automatic data recording. The interesting facts of the first item mentioned, i.e., mathematical consultation, have already been covered earlier in this paper. A few remarks on the other items now follow.

In the field of programming for others it is usually necessary to adopt some general philosophy or mode of operation with respect to the work being performed. Much of this work is of a repetitive nature and many problems can be grouped together in general classes, e.g., trajectories, heat conduction, etc. It is tempting to try to write general computer programs which could be used for each group of problems. Experience has shown, however, that this is usually impossible to do. After careful analysis it has been decided that the best way to proceed is to design a series of flexible subroutines which can be quickly put together in order to carry out the requests of any problem sponsor. This goal has been accomplished in the field of trajectory computations. A series of very flexible subroutines has been coded. These subroutines have made it possible to code and compute a six-degree of freedom trajectory for an unguided rocket within one day. It is hoped that the application of these principles to other problem areas will be equally successful.

In order to make the Naval Ordnance Laboratory "computer minded" and to facilitate getting large numbers of problems coded for the high-speed computer, an extensive training program has been instituted. Over one hundred and fifty people outside of the Mathematics Department have attended courses in programming. The need for additional programming aid is attested to by the large number of major problems with which we have been concerned. Since 1955 between three and four hundred of these problems have been programmed.

As another service to the Laboratory, a group concerned with speeding up and improving data reduction and data recording in the technical areas of the Laboratory has been formed. Thus, if a staff member wants his data reduced quickly by means of a computer, this group can provide or design and build equipment to record or convert the data into the best form for introduction into the computer. This group has just gotten started and promises to speed up many data processing activities at NOL.

Computer Hardware

As a final item, the growth of computing equipment at NOL will be very briefly reviewed. Figure 15 shows our installation several years ago when we had two Card-Programmed-Calculators. These were replaced by two IBM 650's, as shown in Figure 16. As the amount of work went up, these computers were replaced by an IBM 704 as shown in Figure 17. It is to be noted that the 704 is installed in a location quite different from the previous space used. For this we have to thank the Special Projects Office of BuOrd for supplying the necessary funds to prepare this area. We are quite proud of our present installation.

REFERENCES

1. O'Brien, George G., Hyman, Morton A., and Kaplan, Sidney: A Study of the Numerical Solution of Partial Differential Equations. Journal of Mathematics and Physics, Vol. XXIX, No. 4, January 1951.

2. Diaz, J. B.: On an Analogue of the Euler-Cauchy Polygon Method for the Numerical Solution of $u_{xy}=f(x,y,u,u_x,u_y)$. Archive Rat. Mech. Anal. Vol. 1 (1958).

3. Conlan, J.: The Acceleration of Metals by Detonations in one Dimension. NavOrd Report 4332, 20 August 1956.

4. Moore, Robert H.: On Approximations of the Solutions of the Goursat Problem for Second Order Quasi-Linear Equations. Unpublished Thesis for Doctorate Degree at University of Michigan.

5. Squires, R. K., Roberts, R. C., Fisher, E. R.: A Method for Designing Supersonic Nozzles Using the Centerline Mach Number Distribution. NavOrd Report 3995, 10 October 1956.

6. Seeger, R. J., Polachek, H.: On Shock-Wave Phenomena: Waterlike Substances. Journal of Applied Physics, Vol. 22, No. 5, May 1951.

7. Krahn, E.: Negative Magnus Force. Journal of the Aeronautical Sciences, Vol. 23, No. 4, April 1956.

8. Persh, Jerome, Lee, Roland: A Method for Calculating Turbulent Boundary Layer Development in Supersonic and Hypersonic Nozzles including the Effects of Heat Transfer, NavOrd Report 4200, 7 June 1956.

9. Conlan, J.: The Cauchy Problem and the Mixed Boundary Value Problem for a Non-Linear Hyperbolic Partial Differential Equation in Two Independent Variables. Archive Rat. Mech. Anal.

10. Shanks, Daniel: Non-Linear Transformations of Divergent and Slowly Convergent Sequences. Journal of Mathematics and Physics, Vol. XXXIV, No. 1, April 1955.

11. Shanks, Daniel, and Walton, Thomas S.: A New General Formula for Representing the Drag on a Missile over the Entire Range of Mach Number. NavOrd Report 3634, 31 May 1957.

12. Blum, E.K.: Automatic Digital Encoding System II (ADES II). NavOrd Report 4209, 8 February 1956.

Blum, E. K.: Automatic Digital Encoding System II (ADES II), Part 2, NavOrd Report 4411, 29 November 1956.

Blum, E. K., and Stern S.: An ADES Encoder for the 650 Calculator, NavOrd Report 4412, 19 December 1956.

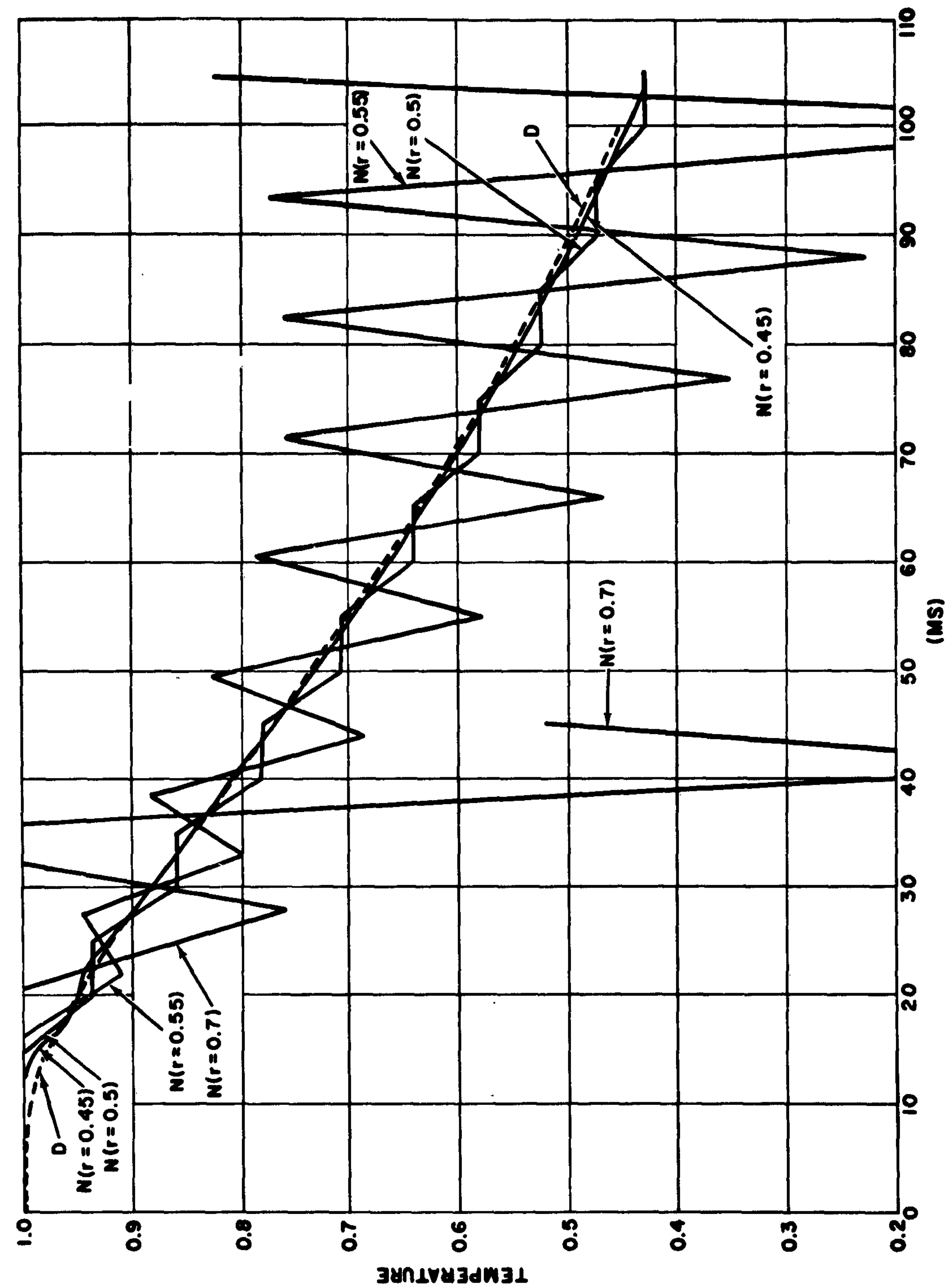


Fig. 1 Stable And Unstable Numerical Solutions Of The Heat Equation

$$u_{xy} = F(x, y, u, u_x, u_y)$$

CAUCHY PROBLEM:

u AND u_n GIVEN ON $y = f(x)$

CHARACTERISTIC BOUNDARY VALUE PROBLEM:

$$u(x, y_0) = s(x) \quad , \quad u(x_0, y) = t(y)$$

MIXED PROBLEM:

$$u(o, y) = s(y) \quad , \quad u(x, x) = t(x)$$

Fig. 2 Various Initial Value Problems For A Non-Linear Hyperbolic Partial Differential Equation

REFLECTION PATTERN AFTER
INCIDENT SHOCK HITS
THE CORNER A

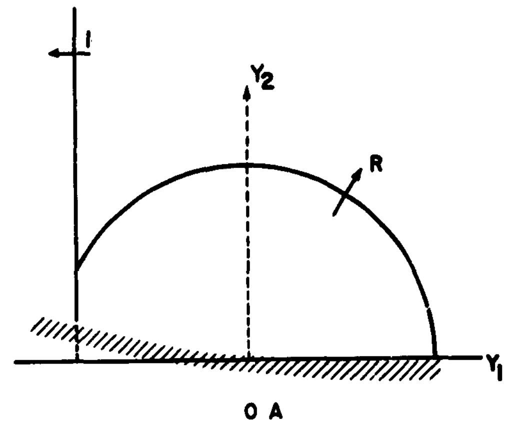


Fig. 3 Plan Shock Incident On A Corner

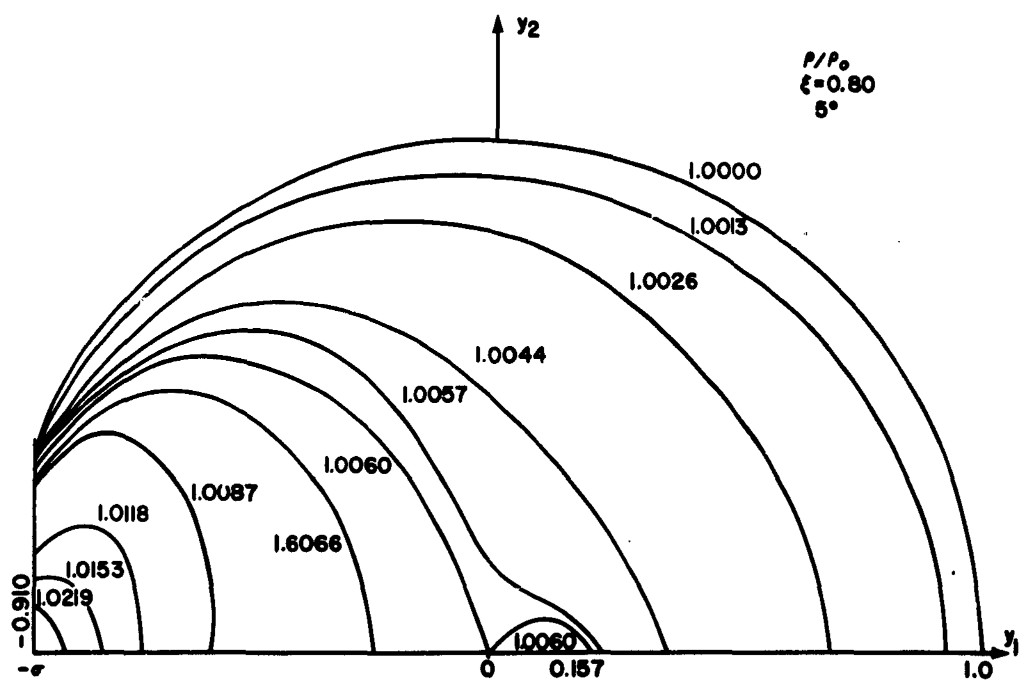


Fig. 4 Constant Pressure Curves For Incident Shock

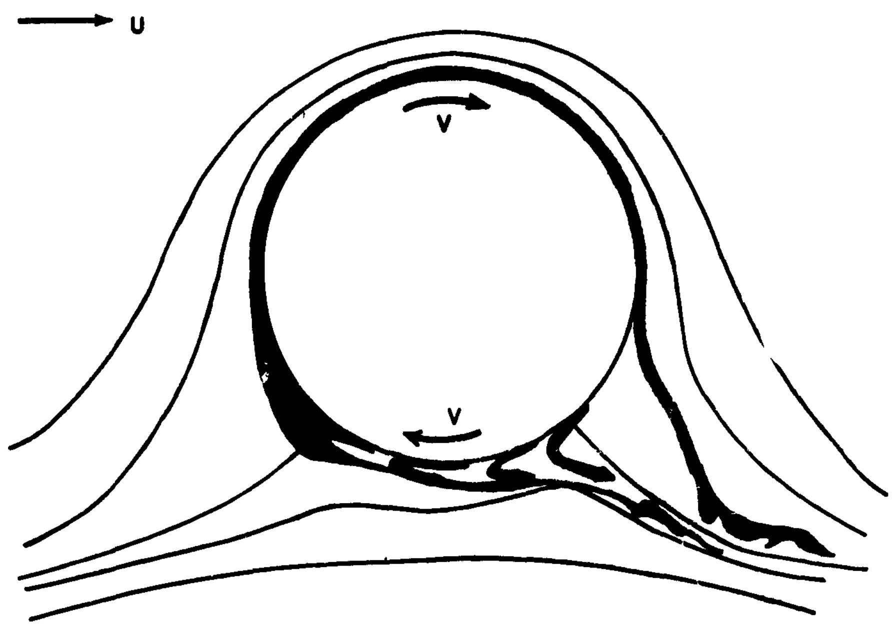


Fig. 5 Spinning Cylinder In Cross-Flow

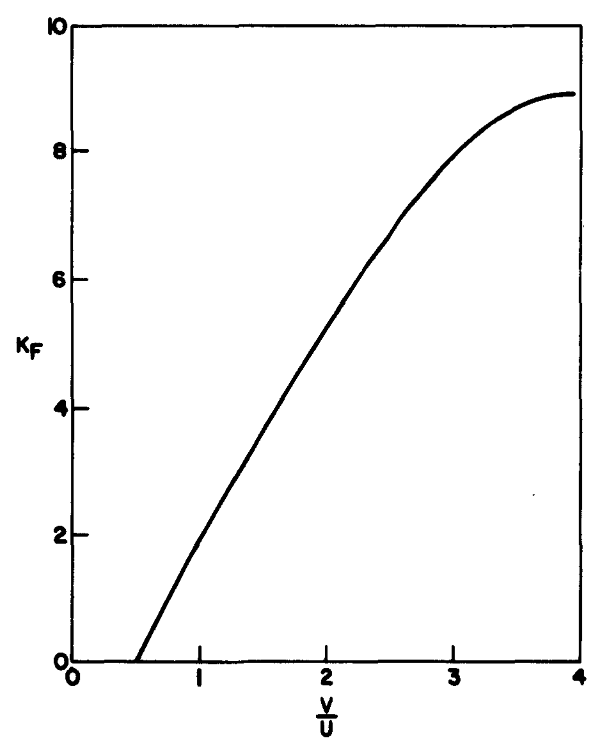


Fig. 6 Force Coefficient As A Function Of Velocity Ratio

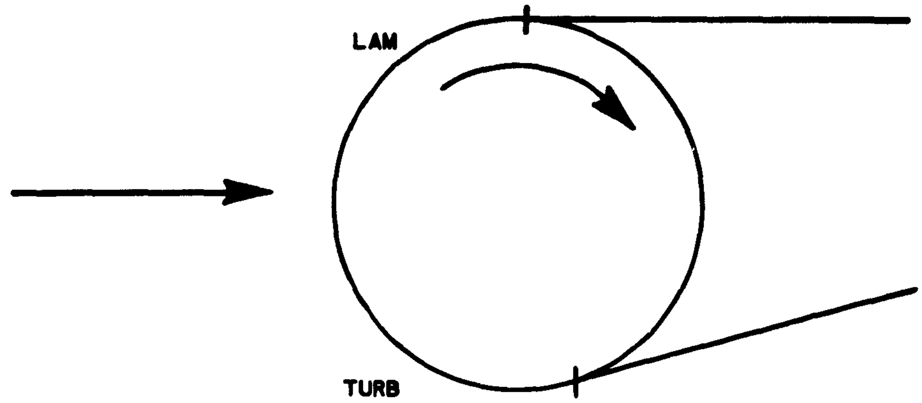


Fig. 7 Explanation Of The Flow Situation Causing Negative Magnus Force

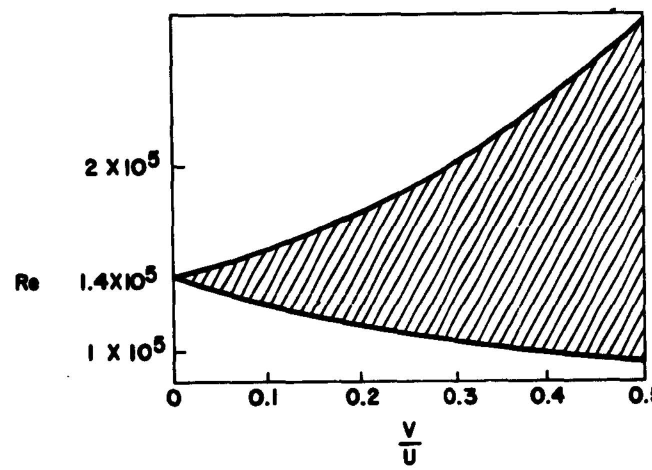


Fig. 8 Region Where Negative Magnus Force Will Appear

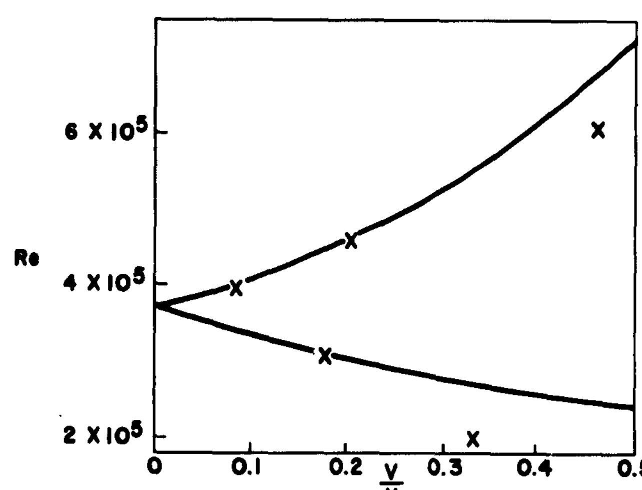


Fig. 9 Comparison With Experimental Data

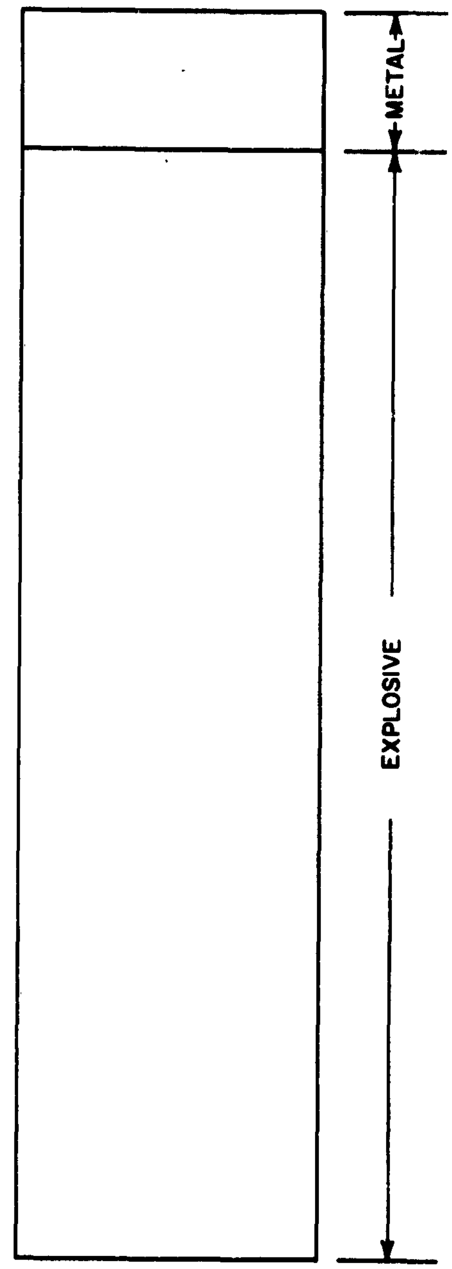


Fig. 10 Explosive And Metal Combination

GIVEN THE SEQUENCE

$$A_o, A_1, A_2, \ldots, A_n, \ldots$$

WE DEFINE A NEW SEQUENCE

$$B_{k,o}, B_{k,1}, B_{k,2}, \ldots, B_{k,n}, \ldots$$

WHERE

$$B_{o,n} = A_n \quad (n = 0, 1, 2, \ldots)$$

Fig. 11 Non-Linear Transformation Of Sequences

$$B_{k,n} = \frac{\begin{vmatrix} A_{n-k} & \cdots & A_{n-1} & A_n \\ \Delta A_{n-k} & \cdots & \Delta A_{n-1} & \Delta A_n \\ \Delta A_{n-k+1} & \cdots & \Delta A_n & \Delta A_{n+1} \\ \vdots & & \vdots & \vdots \\ \Delta A_{n-1} & \cdots & \cdot & \Delta A_{n+k+1} \end{vmatrix}}{\begin{vmatrix} 1 & \cdots & 1 & 1 \\ \Delta A_{n-k} & \cdots & \Delta A_{n-1} & \Delta A_n \\ \Delta A_{n-k+1} & \cdots & \Delta A_n & \Delta A_{n+1} \\ \vdots & & \vdots & \vdots \\ \Delta A_{n-1} & \cdots & \cdot & \Delta A_{n+k-1} \end{vmatrix}}$$

Fig. 12 Non-Linear Transformation Of Sequences

n	A_n	B_n	C_n	D_n
0	4.0000000			
1	2.6666667	3.1666667		
2	3.4666667	3.1333333	3.1421053	
3	2.8952381	3.1452381	3.1414502	3.1415993
4	3.3396825	3.1396825	3.1416433	
5	2.9760462	3.1427129		
6	3.2837385			

$$\pi = 4 - 4/3 + 4/5 - 4/7 + \ldots$$

Fig. 13 Application Of Shank's Transform To Sum A Slowly Convergent Series

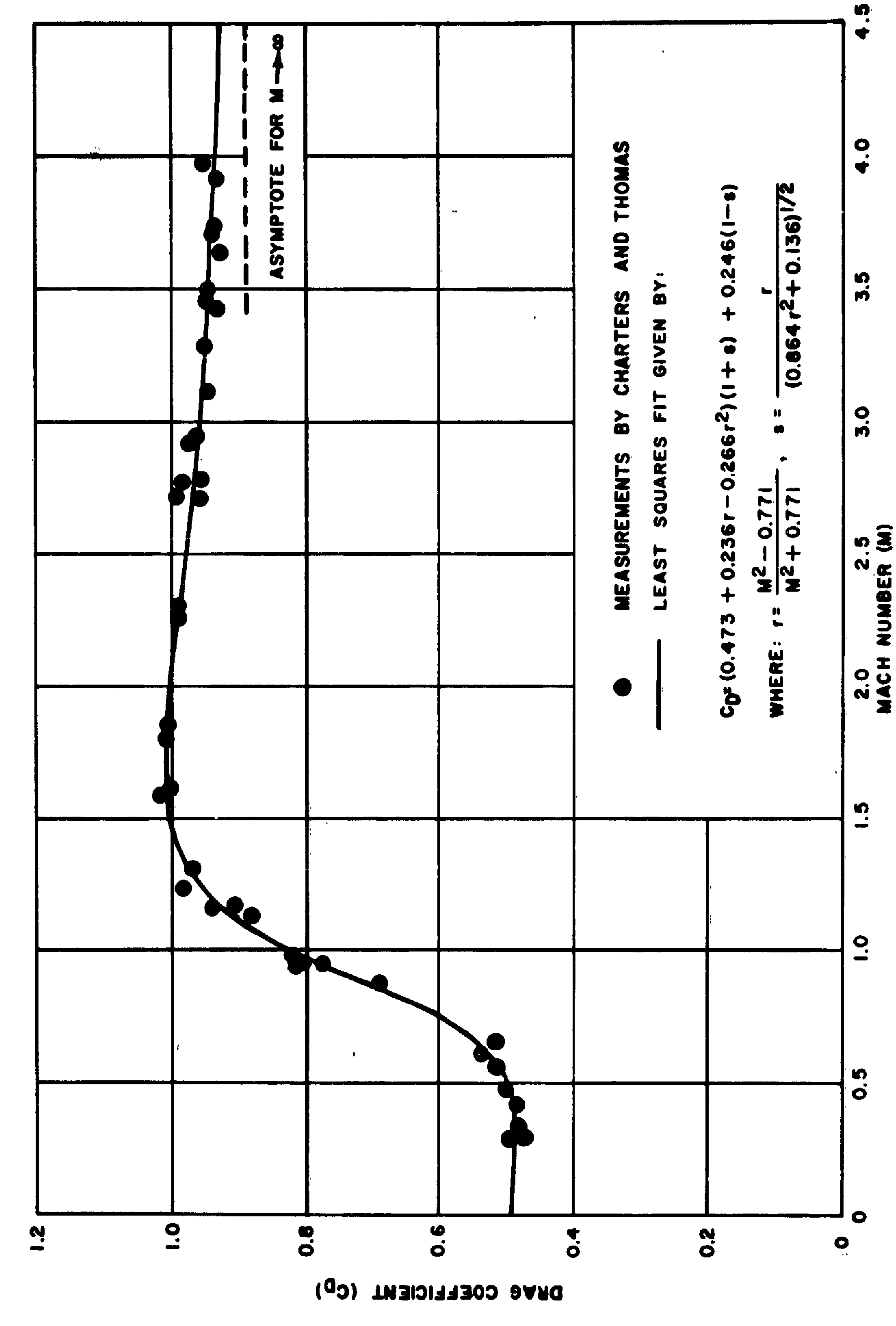


Fig. 14 Drag Corfficient VS Mach Number For Small Spheres

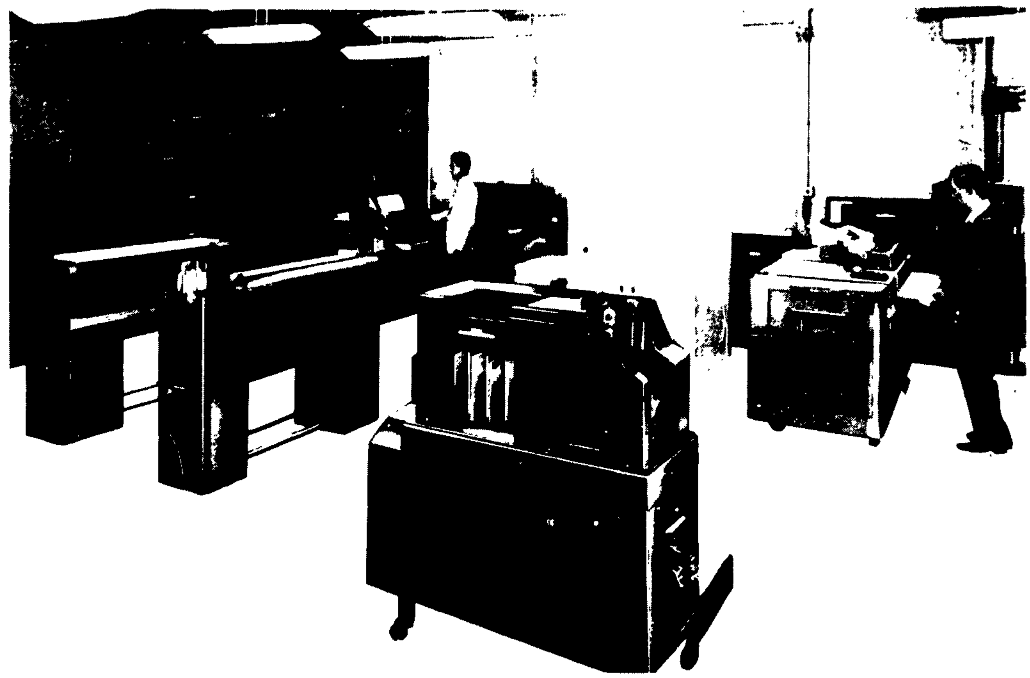


Fig. 15 Card-Programmed-Computer Installation At NOL

Fig. 16 IBM 650 Installation At NOL

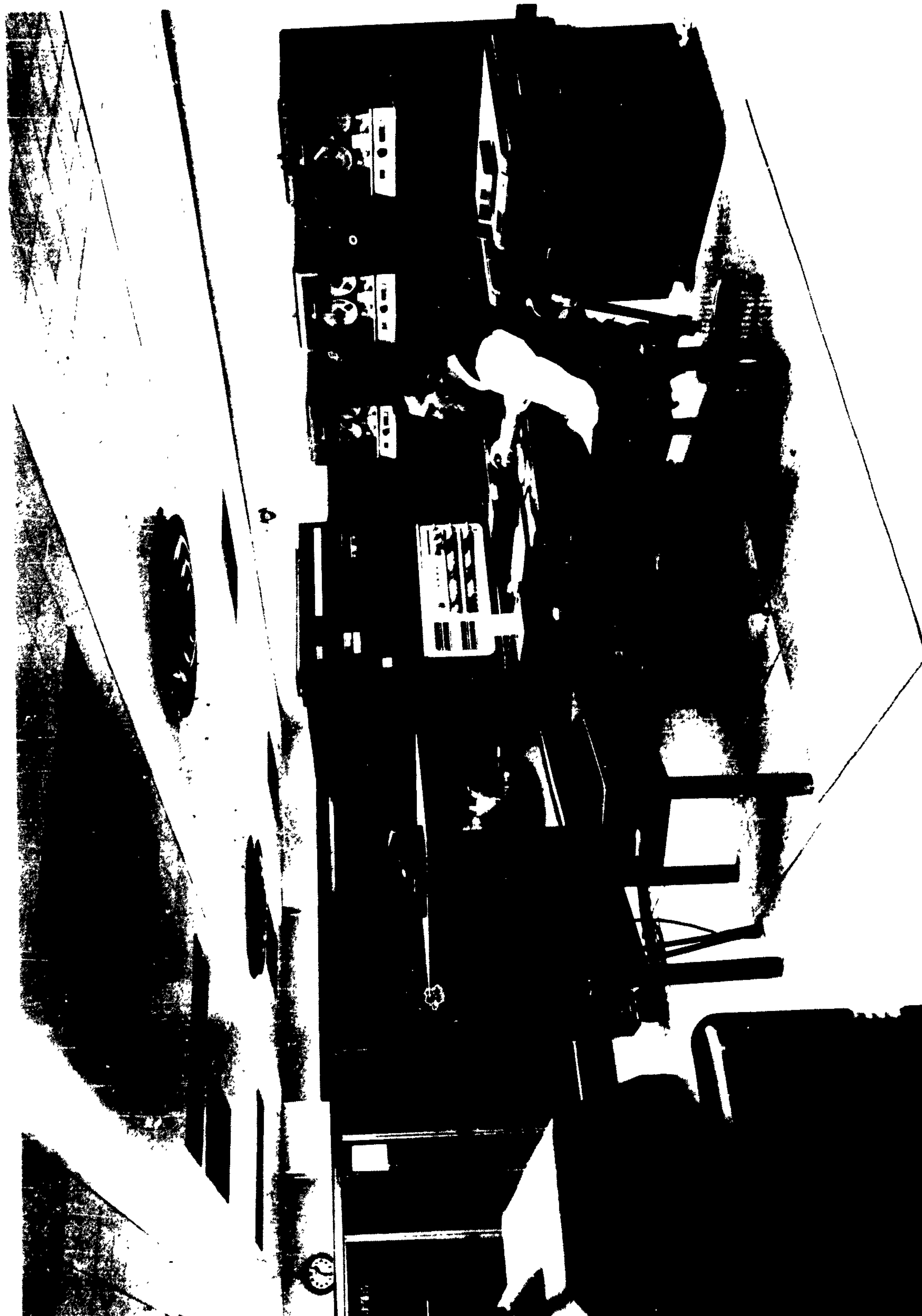


Fig. 17 IBM 704 Installation At NOL

EFFECTS OF MASS TRANSFER ON BOUNDARY LAYER CHARACTERISTICS

by

I. Korobkin
Naval Ordnance Laboratory

Of the various techniques proposed for protecting high-speed vehicles from aerodynamic heating, mass-transfer cooling appears to be among the most promising. The topic of mass-transfer cooling can be divided into two general categories, mass injection into the boundary layer and mass ablation, and I shall restrict my presentation to boundary-layer injection. (Any future references to mass transfer will really mean boundary-layer injection.)

Though considerable effort has been expended in recent years on the mass-transfer approach, it is not as yet completely understood, certainly from a quantitative standpoint. The phase in which the greatest progress has been made is the theoretical study of the laminar boundary layer, first with air injection into an air boundary layer and to a lesser extent with foreign gas injection. However, the theoretical studies are contrasted by a dearth of experimental confirmation. The turbulent boundary-layer case has been studied mathematically to a far lesser extent than the laminar boundary layer and is subject to the kinds of approximations, hypotheses, and empiricism which have plagued all previous turbulent boundary-layer treatments for the past half-century. A few experiments with turbulent boundary-layer mass transfer have been described in the literature, but, unfortunately, they do little to amplify our understanding of the subject, particularly of the compressible flow case. The least understood topic of all is the effect of mass injection on boundary-layer stability and transition.

It would take considerably longer than the time allocated to me to give a detailed description of the world-wide effort in mass-transfer cooling, and so I shall limit my remarks to a discussion of Naval Ordnance Laboratory efforts in the field, much of the work still unpublished.

NOL has made theoretical studies of the laminar boundary layer, of the turbulent boundary layer, and of boundary-layer stability, and is currently conducting experiments with turbulent boundary layers. All the work at NOL, as elsewhere, indicates that skin friction and heat transfer are reduced by the injection of gases into the boundary layer. The question immediately arises as to which gas is most effective in reducing heat transfer. Since weight is a principal factor in the design of missiles, the criterion for effectiveness is normally established as the relative reduction in heat transfer for a given mass injection rate of foreign gas.

An analysis of the steady laminar boundary layer over a flat plate with foreign gas injection was published by NOL in May 1956 (reference 1). The report suggested various mathematical techniques for solving the boundary-layer equations for momentum, energy, and diffusion but did not include any numerical results. Similar studies, e.g. reference 2, made elsewhere did include numerical results, and the principal conclusion which was reached by most, if not all, investigators was that the smaller the molecular weight of the injected gas, the greater its effectiveness. Upon examination of the various treatments it was immediately evident that the conclusion regarding the effectiveness of the low molecular weight gas was based on an analysis of just a very few gases such as H_2, He, N_2, and CO_2 as compared to air. Furthermore, the molecular weight is the only characteristic which forms a systematic pattern for these gases. The He molecule is heavier than H_2 but has a smaller collision or molecular diameter than H_2 and is monatomic, whereas H_2 is diatomic. CO_2 is the largest in all respects. It seemed to us at NOL that a closer look at the fundamental properties of the molecule was in order before a positive statement could be made regarding the characteristics of a gas resulting in the greatest mass-transfer effectiveness.

When the compressible laminar boundary-layer equations are set up on a mass basis, i.e., fluid quantities are described in terms of pounds or grams rather than mols, the simplest formulation is achieved by considering a flat plate with injection varying as $1/\sqrt{x}$ and by assuming an ideal gas equation of state. In this case the characteristics of the injected gas are incorporated into the analysis in terms of specific heat, the transport properties of viscosity, thermal conductivity, and diffusion coefficient, and by the gas constant. (If the analysis were made on a molar basis, the gas constant would be identical for all gases.)

The above five parameters can be reduced to three if the simple rigid sphere model is selected for the molecular collision processes. The transport properties can be expressed in terms of molecular weight and diameter, and the gas constant can be expressed in terms of molecular weight and the universal gas constant. The specific heat is still retained as a parameter, but the number of atoms in the molecule can be studied by determining the molar specific heat from the equipartition energy, which describes specific heat in terms of the degrees of freedom of the molecule.

For purposes of analysis, the molecular weight and molecular diameter of He, air, CCl4 vapor were selected from tables, and the specific heats were computed in accordance with the equipartition of energy for monatomic, diatomic, and polyatomic gases at low temperatures with a minimum of vibrational excitation. All possible combinations of molecular diameter, molecular weight, and specific heat were selected to form 27 gases, three which are real and 24 which are hypothetical (see Figure 1). These 27 gases were assumed as the foreign gas for injection into a compressible laminar boundary layer, and the equations were solved on the NORC high-speed computer at the Naval Proving Grounds, Dahlgren, Virginia. The effects of injection on skin friction are demonstrated in Figures 2, 3, and 4. For purposes of illustration only a single environment has been selected, i.e., M 6 and ratio of wall to free-stream temperature of 0.5. In each case two of the three parameters of molecular weight, molecular diameter, and specific heat, were kept constant and the third was varied systematically.

Figure 2 indicates the effect of specific heat for the cases where the molecular weight and molecular diameter are identical to those of actual air. As one may have anticipated, the specific heat has little effect on skin friction. There is, however, a slight advantage to use of low specific heat gases. Figure 3 illustrates the effect of molecular weight at constant molecular diameter and specific heat. The expected desirability of low molecular weight gases is emphatically demonstrated, in agreement with the work of other investigators.

The most interesting skin-friction result is the effect of molecular diameter illustrated in Figure 4. The larger the molecular diameter the more effective the gas. Remember that large molecular diameters are associated with heavier molecules. The question immediately arises as to which of the two parameters, molecular weight or molecular diameter, is more significant. Past results obtained by other investigators who studied only a few actual gases, indicate that m should be more important, but I will show later that this idea is not always true.

The heat-transfer results are not as straightforward as in the case of skin friction where $\tau_w = \left[\mu \left(\frac{\partial u}{\partial y}\right)_w\right]$. The energy flux across the fluid-solid interface is no longer given simply by $-\left(k \frac{\partial T}{\partial y}\right)_w$. Because of diffusion and injection (convection) the energy transport across the interface, i.e., the energy which will increase the missile temperature, is given by $E \quad -\left(k \frac{\partial T}{\partial y}\right)_w + \left(h_1 \rho V\right)_w$ where h_1 is the specific enthalpy of the injected gas. Since the enthalpy is related to the specific heat, at high rates of injection the specific heat will be the dominant factor. Furthermore, since $h_1 \rho V$ is opposite in sign to $k \frac{\partial T}{\partial y}$, the convective term tends to reduce the heat transfer, making a high specific heat desirable. Of course, at low rates of injection the conduction term, $k \frac{\partial T}{\partial y}$, is still important. At low injection rates the important parameters are once again low molecular weight, large molecular diameter, accompanied by high specific heat for the convection. Since methane (CH_4) has a fairly large molecular diameter, is polyatomic, and has a fairly low molecular weight, it was selected for comparison with helium. The comparative properties at a temperature of $0^{o}C$ are given in the table below:

	He	CH_4
m	4	16
d	$2.6A^{o}$	$3.82A^{o}$
c_p(mol)	2.5Ru	4.25Ru
c_p(mass)	0.625Ru	0.267Ru

where the molecular diameter is specified in Angstrom units, and Ru is the universal gas constant.

A comparison of the relative effectiveness for energy transfer of He and CH_4 at low rates of injection is given in Figure 5. The results indicate that for low blowing rates methane is superior to He despite the lower specific heat and higher molecular weight associated with CH_4, illustrating the importance of the molecular diameter. Since a large molecular diameter is a characteristic of polyatomic molecules, a closer look was taken at polyatomic molecules to discover any advantageous properties that could be applied for higher blowing rates. As stated earlier, at high rates of injection the dominant factor is the specific heat on a mass basis, i.e., the molar specific heat divided by the molecular weight. Helium has a high mass specific heat by virtue of its low molecular weight, whereas its molar specific heat is low as in the case of all monatomic gases. Polyatomic gases have contributions to their molar specific heat from rotation of the molecules and from vibrational excitation at elevated temperatures. For example, methane at room temperature has a molar specific heat of 4.25Ru whereas at $1000^{o}K$ the molar specific heat is 8.6Ru, and its mass specific heat is 0.538Ru or about the same as He with its value of 0.625Ru.

Furthermore, at this temperature only about one-half the possible vibration modes for methane have been excited. The comparative molar specific heats are given in the table below:

MOLAR SPECIFIC HEAT (c_p)

Temperature	He	CH_4
300°K	2.5Ru	4.25Ru
480°K	2.5Ru	5.5 Ru
1000°K	2.5Ru	8.6 Ru
Possible c_p for CH_4 with all vibrational modes excited		13 Ru

When a mass-transfer system is used to protect a missile the missile surface will operate at its maximum safe temperature to minimize the amount of fluid injected. Therefore, temperatures at which the injected gas could be appreciably excited vibrationally do not seem unreasonable. An actual study of the effects of vibrational excitation on the boundary-layer equations has not been performed as yet by NOL, but one is planned for the near future.

As far as the turbulent boundary layer is concerned, NOL studies have been made only of the air to air case, but experiments with foreign gas injection are planned for this summer. Boundary-layer profiles were determined for the turbulent boundary layer on a flat plate with uniform air injection, and skin-friction coefficients were calculated from the slope of the velocity profile at the wall. The results were correlated on a Re_θ basis and are compared with the prediction of Rubesin of NACA (reference 3) in Figure 6. The local skin-friction coefficient is plotted on a non-dimensional basis, the zero injection value being used as the denominator. The band identified as the theoretical work of Rubesin corresponds to the Re_θ's of the measurements. On the basis of this correlation the Rubesin theory seems to describe the measurements quite satisfactorily. A similar analysis made at NOL (reference 4) was overly optimistic as to the effectiveness of the mass injection.

When the same data are compared to the Rubesin theory on an absolute basis, Figure 7, we see that the Rubesin prediction is somewhat pessimistic. Comparison of the various assumptions and hypotheses made by Rubesin with the actual details of the measured profiles indicates serious discrepancies. The above results point out the well-known weaknesses in turbulent boundary-layer analysis. I hope that the NOL data and other detailed profile measurements can be used to formulate a more physically realistic theory.

I will conclude my presentation with one brief remark on the effect of injection on boundary-layer stability. Studies at NOL (reference 5) and elsewhere have shown that injection is a destabilizing effect, and for a given wall temperature the light weight molecule is more destabilizing than the heavy molecule. This fact should be considered together with the previous discussion of the laminar boundary layer in selecting gases for boundary-layer injection.

REFERENCES

1. Shen, S. F., The Steady Laminar Boundary Layer over a Flat Plate with Injection of a Different Gas: NOL NavOrd Report 4235, May 1956

2. Baron, J. R.: The Binary-Mixture Boundary Layer Associated with Mass Transfer Cooling at High Speeds: MIT Naval Supersonic Laboratory Technical Report 160, May 1956

3. Rubesin, M. W.: An Analytical Estimation of the Effect of Transpiration Cooling on the Heat-Transfer and Skin-Friction Characteristics of a Compressible Turbulent Boundary Layer, NACA TN 3341, December 1954

4. Persh, J.: A Theoretical Investigation of the Effect of Injection of Air on Turbulent Boundary-Layer Skin-Friction and Heat-Transfer Coefficients at Supersonic and Hypersonic Speeds, NOL NavOrd Report 4220, January 1957

5. Shen, S. F.: The Theory of Stability of Compressible Laminar Boundary Layers with Injection of a Foreign Gas, NOL NavOrd Report 4467, May 1957

	GAS 1 He	GAS 2 AIR	GAS 3 CCl_4
MOLECULAR WEIGHT	m_1	m_2	m_3
MOLECULAR DIAMETER (A°)	d_1	d_2	d_3
SPECIFIC HEAT (ON UNIT MASS BASIS)	c_{p_1}	c_{p_2}	c_{p_3}

Fig. 1 Matrix Of 27 Gases For Boundary Layer Injection

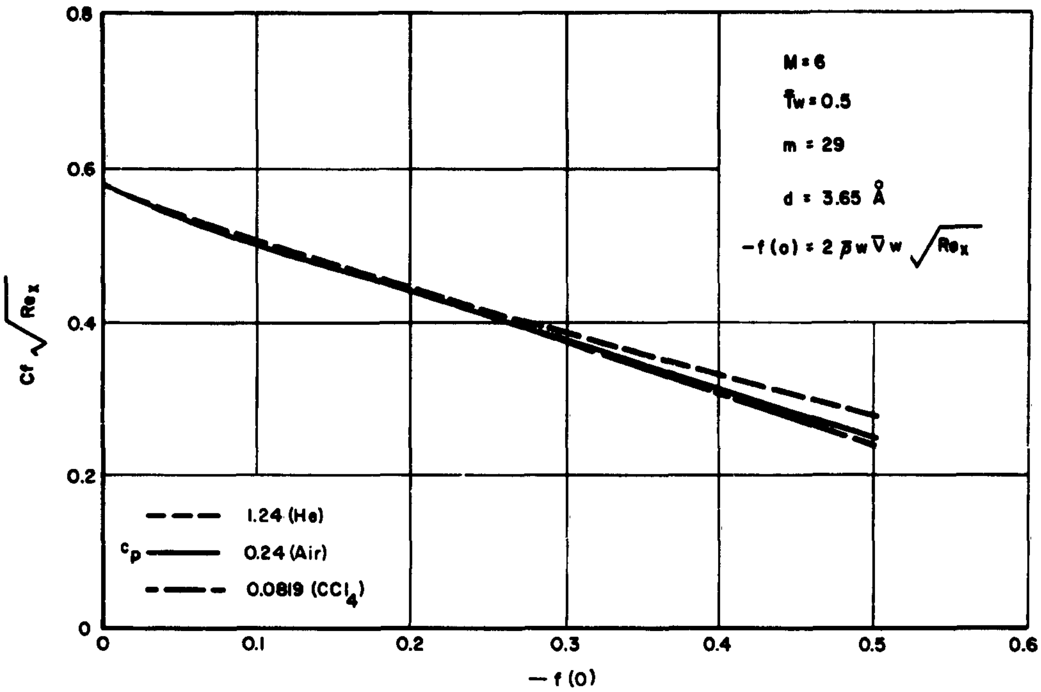


Fig. 2 $C_f\sqrt{Re_x}$ VS Foreign Mass Injected. Effect of c_p

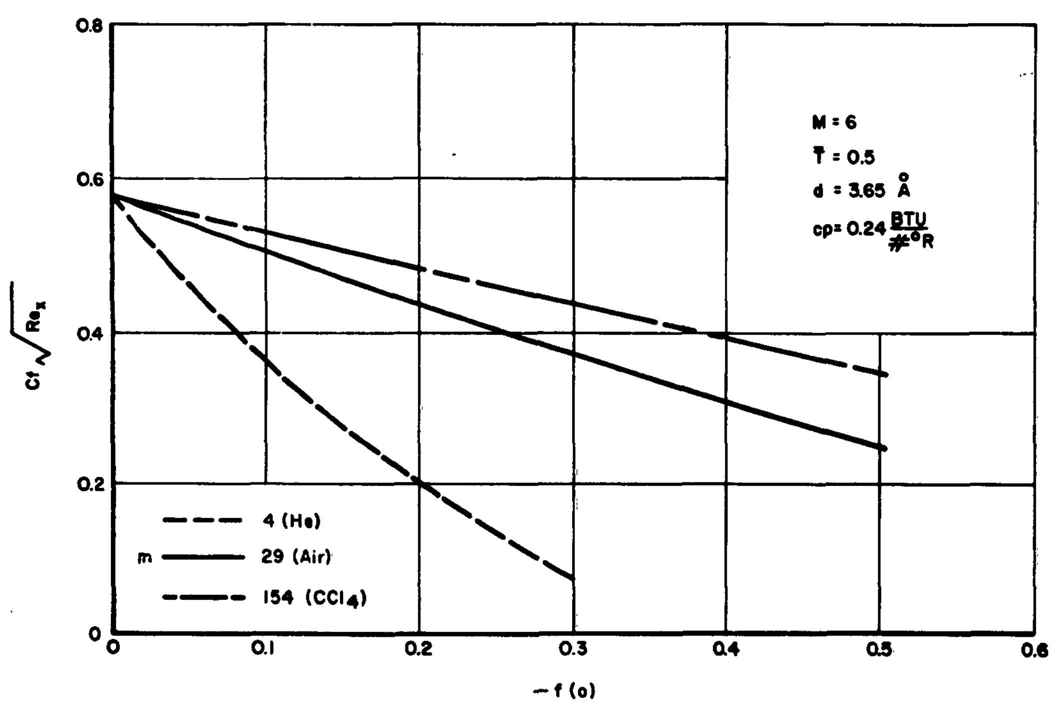


Fig. 3 $C_f\sqrt{Re_x}$ VS Foreign Mass Injected. Effect of m.

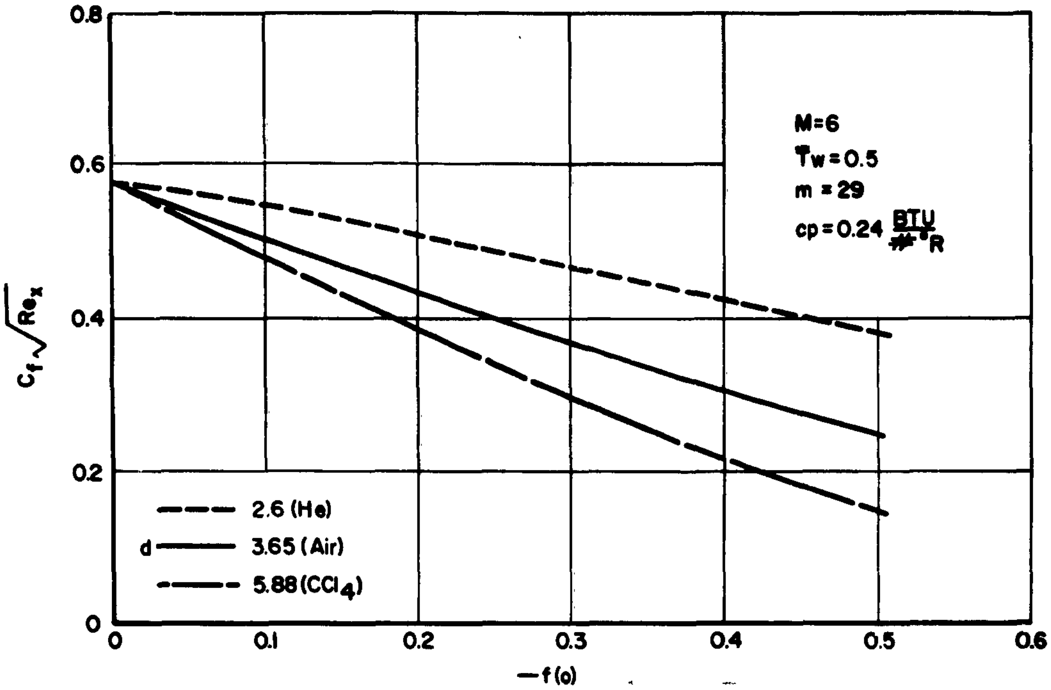


Fig. 4 $C_f\sqrt{Re_x}$ VS Foreign Mass Injected. Effect of d.

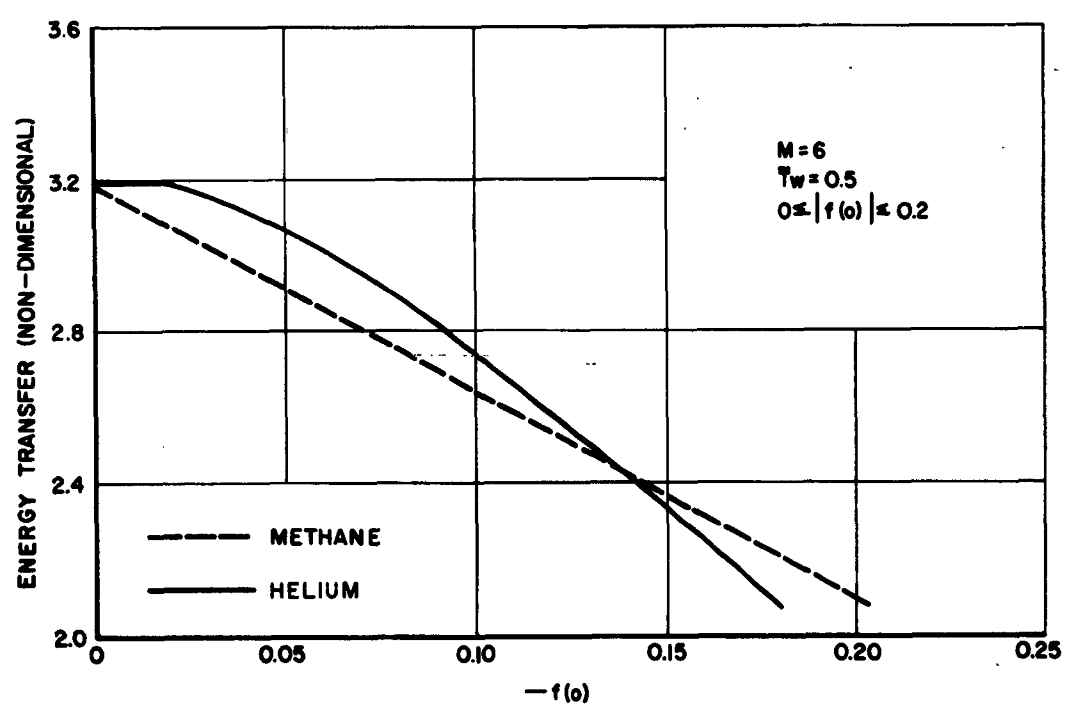


Fig. 5 Comparison Of Energy Transfer For Helium And Methane For Low Rates Of Blowing

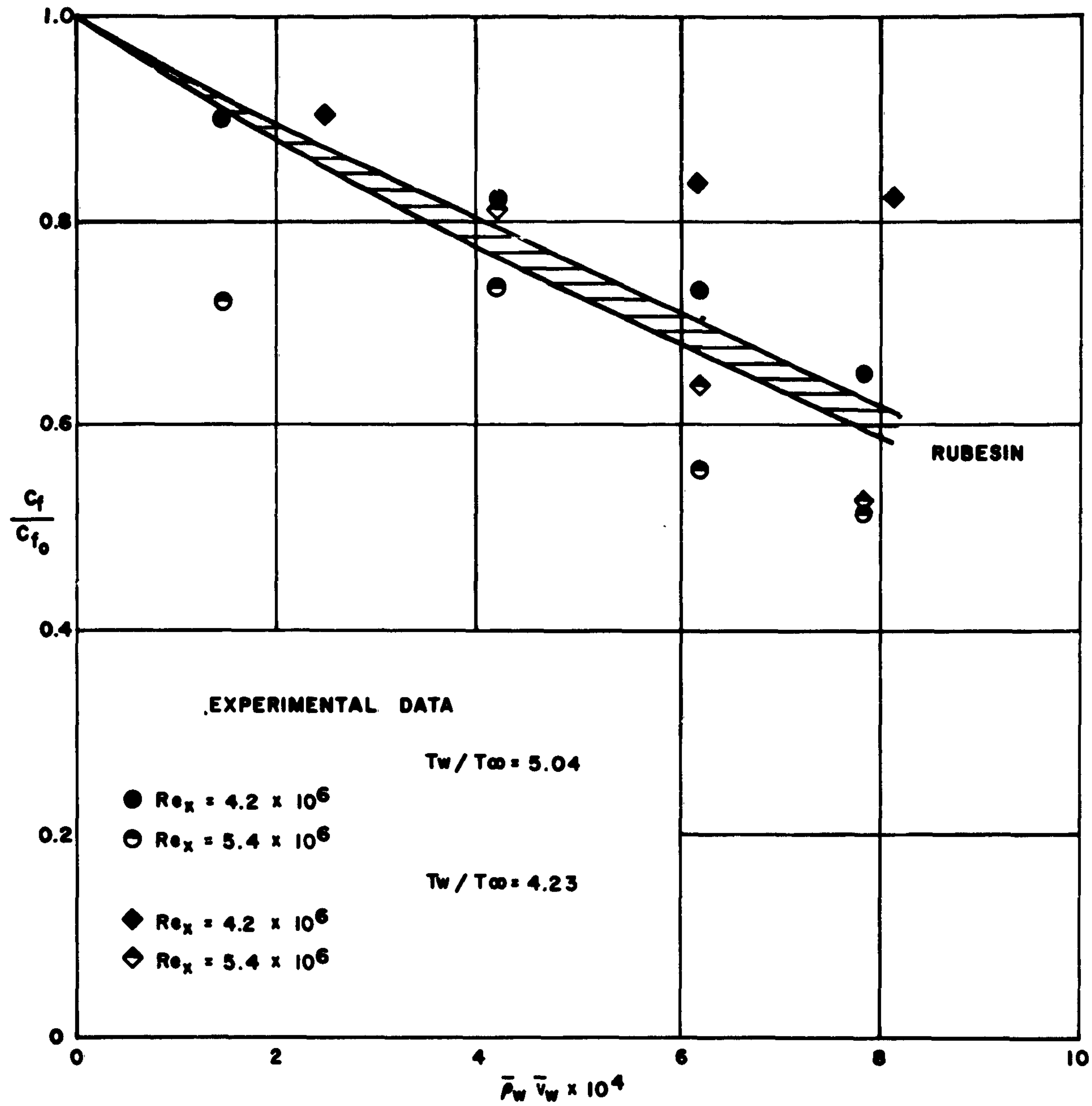


Fig. 6 Variation Of Skin-Friction With Air Injection Rate At $M_\infty \approx 5{:}08$

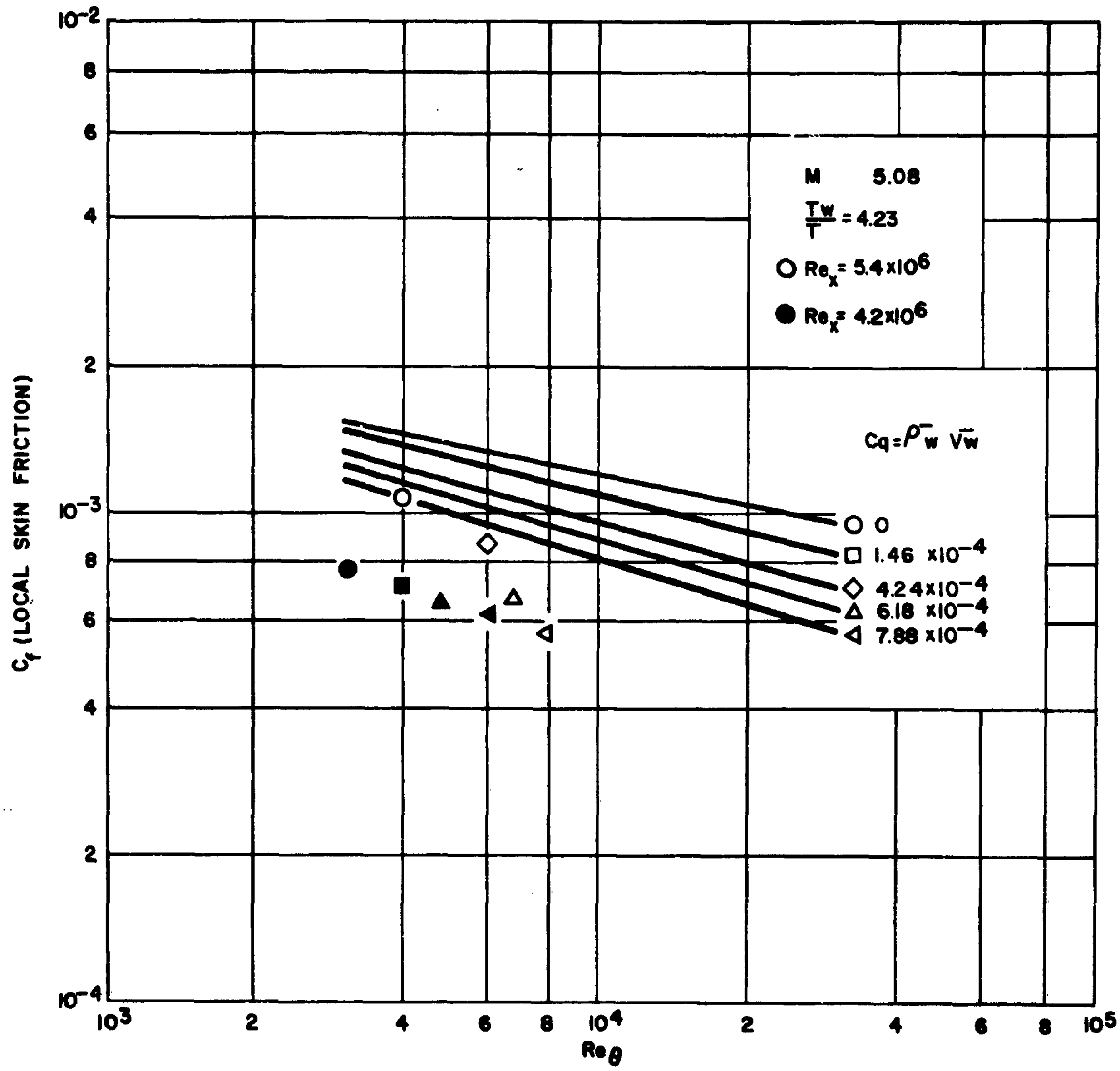


Fig. 7 Effect Of Blowing On Skin Friction Of A Turbulent Boundary Layer

SPECIAL AFTER-LUNCHEON ADDRESS

by

Dr. Wernher von Braun
Director, Development Operations Division
Army Ballistic Missile Agency

Ladies and Gentlemen:

Let me first apologize for apparently having fouled up a well-planned schedule, but my time today is, unfortunately, rather limited. Originally I wasn't even expected to give a talk about rockets, but merely a little non-technical luncheon address. Well, things have changed, so I thought I would tell you in a very informal way a little bit about what we have done at the Army Ballistic Missile Agency in the recent past, and even more importantly, what we plan to do in the immediate future. Aerodynamics and missile development have been in a continuous race for years and you aerodynamicists probably know even better than we missile people that Mach numbers of Wind Tunnels and Ballistic Ranges have an unfortunate way of trailing behind Mach numbers of missiles.

I just saw your new hypersonic wind tunnel and your ballistic range. The unfortunate thing is - proud and happy as all of you can be for having now a fine facility capable of Mach 10 - that this is simply less than we need. It is less than what we have reached already with our missiles and is far less than what we think we will have in the very near future. So I thought by telling you what we are planning these days, I may at least get you a little bit excited about Mach numbers well beyond the range that you are dealing with now.

I brought a few slides along, and like any good television show, I'd like to start out with a commercial. May I have Figure 1, please?

This "commercial" serves to remind you what our Juno II Missile looked like that we used on the 3rd of March 1959 to put a satellite around the Sun. This Juno II rocket is essentially a Jupiter IRBM with a three-stage cluster of solid rockets mounted in its nose. The entire cluster was developed and built by our friends and teammates from the old Explorer program at the Jet Propulsion Lab in Pasadena. This total JPL cluster of solid rockets which can be seen in the foreground, red being the second stage, blue the third, and yellow the fourth stage, weighs only approximately 1000 pounds which is far less than the payload of the standard Jupiter IRBM. As a result, we were able to place the equivalent of the payload thus saved in the form of additional propellants into the first stage -- the Jupiter itself. Thus we increased the burning time of the first stage from that of the standard Jupiter to a full three minutes or 180 seconds. After burnout of the first stage, we separate the nose from the booster. The nose begins where the cylindrical shape of the first stage meets the cone frustrum and it is at this point, where we now separate the nose section. With the help of a jet nozzle attitude control system, we then turn this nose section with its spinning cluster into the right spatial attitude so that the solid rockets can be fired into the correct direction. Simultaneously, the conical tip of the aerodynamic shroud is jettisoned. At a given instant, the second stage fires. It is made up of eleven "eighth-inch scale Sergeants." They burn for about five seconds. We then have a two-second coasting period after which we fire the third stage of three "scale Sergeants" which had been inserted inside the second-stage cluster of eleven. Finally, we fire the fourth stage which sits on top of the three. Only the fourth stage, of course, and the little payload of about 15 pounds weight in the tip of the fourth stage, reached the necessary speed to escape from the gravitational field of the Earth and enter an orbit around the Sun.

We called our two Juno II rockets "lunar probes" because we had not only the Sun but also the Moon in mind when we fired them. The beauty about aiming at the Moon is that if you miss it, the probe will always go in orbit around the Sun. So you can't really lose. We knew in advance that with such a crude spin-stabilizing mechanism for the top stages we didn't have much of a chance of hitting the Moon which, after all, subtends only over an angle of about half a degree and it does not stand still, either. So, we waited just for the right firing moment and picked a trajectory which would intercept the Moon. But we knew in advance that we didn't really have more than one chance in 10,000 of hitting the Moon. We thought we might get close to it, though, and that is what we actually did.

Let me say a few words about trajectories to the Moon in this connection. You read a lot of nonsense in the newspapers to the effect that you can fire at the Moon only on a given date when the Moon is particularly close to the Earth. Actually this has nothing to do with the selection of the firing date. The Moon goes around the Earth in a very nearly circular orbit and the little difference between perigee and apogee of the Moon is really not worth mentioning. What is important is that you fire under a condition where you can make the fullest possible use of the circumferential velocity of the launching site. Due to the Earth's rotation any point on the Equator moves from west to east at a rate of approximately 1500 feet per second. Even at Cape Canaveral's location on about 28 degrees Northern Latitude, you have about 1200 feet per second eastbound velocity. Remember now that this is the equivalent of a supersonic booster! If you fire from Canaveral in a northern or southern azimuth over the poles, you just lost the benefit of that initial velocity, and this you have to pay for dearly in payload. So for a rather rudimentary missile like a Juno II, which had not much payload to begin with, we had carefully selected a day at which we could fire at the Moon approximately under an azimuth due East.

Another important consideration is the following: For energy reasons the geocentric angle (which is an angle whose vertex is located in the center of the Earth) between the injection point into the unpowered transfer trajectory and the point where the missile intercepts the Moon, should be as large as possible. By that I mean the following: you all know that the minimum energy transfer trajectory is a so-called Hohmann ellipse whose apogee would just be tangent with the Moon's orbit. Such an ellipse would have a geocentric angle, or a central angle as we call it, between the injection point and the contact point with the Moon of exactly 180 degrees. Such Hohmann ellipses, or minimum energy ellipses, are the most economical way of transferring an object from the Earth to the Moon. They also have the longest flight time of all conceivable transfer paths.

Now, it is not too advantageous to select a Hohmann Ellipse when you are dealing with a rudimentary missile like this Juno II, simply because you cannot control your terminal velocity of the fourth stage accurately enough. Any error in that velocity affects the apogee location and the transfer time tremendously so that your chances of even getting close to the Moon are very greatly impaired. For that reason we selected a hyperbolic transfer. The hyperbolic transfer has a much smaller central angle than 180 degrees but it is still pretty large. We actually selected one of about 120 degrees. Of course, the smaller the central angle gets, the higher must be the speed at which you inject (for a central angle zero the speed would be infinite).

When your firing site is located on the Northern Hemisphere (like Cape Canaveral, 28 degrees northern latitude), you can transfer through a trajectory of 120 degrees central angle only at a time when the Moon, at the time of firing, is on the Southern Hemisphere. This is simply due to the fact that the projection of all such transfer paths onto the Earth's surface must be great circles, because one focus of the transfer hyperbola is in the center of the Earth. You can transfer an object from the Earth to the Moon only through trajectories whose projection onto the Earth's surface are great circles. To make the geocentric angle of this great circle 120 degrees the Moon must obviously be over the Southern Hemisphere while you are firing from the Northern Hemisphere.

These two requirements - due East firing Azimuth and Moon's position in the Southern Hemisphere - then had to be combined to determine the firing date. It so happened that during the winter 1958/59 this condition existed during the early days of every month. That's why we fired our first Juno II in early December 1958, and the second one, which was fully successful and went in orbit around the Sun, on the third of March 1959.

In the case of our first try, Juno II No. 1, which later became known as Pioneer III, our velocity was three per cent shy of escape velocity. We in Huntsville, of course, were very sad and disappointed about this, because everything else had worked so well. But there was at least one member in our family who was happy, and that was Dr. James Van Allen. Jim had his Geiger counters in our JPL-built payload. The idea had been to use this "lunar probe" among other things to determine the radiation intensity of the Van Allen Belt at great distances from the Earth, and to find out if the Moon might have her own little Van Allen Belt. Well, the way things went with Pioneer III, Jim Van Allen got all his measurements not only on the way up as planned, but he also got another set of completely unscheduled measurements on the way down. So he was very happy. In fact, he was awfully happy. For, due to the interaction between the relatively straight trajectory of the probe on the one hand and the Earth rotation on the other, there had been gaps in his measurements on the ascent during such periods where line of sight contact between our ground stations on the Western Hemisphere and the probe was interrupted. After Pioneer III had risen to an altitude of 65,000 miles, it so happened that on the way down line of sight contact and signal communication was maintained at the very altitudes at which Jim's measurements were interrupted on the way up. Thus he got a complete uninterrupted record of trapped radiation intensity all the way up to 65,000 miles altitude.

Could I have Figure No. 2 please? This figure shows the payload package. You see the two Geiger counters here that I just mentioned. These small cylinders are mercury cells, our form of payload power supply. In here in the 960 megacycle transmitter which transmitted the messages from the Geiger counters and which also served to track the probe. The little rod up here is the antenna. Now you know that our second attempt with a Juno II, which afterwards became known as Pioneer IV, was fully successful from the performance standpoint also. Its terminal speed exceeded escape velocity and thus Pioneer IV became the Free World's first artificial planet of the Sun. With this tiny antenna and this humble little transmitter, having a transmitting power of a meager 165 milliwatts, JPL managed to maintain contact with the probe over a distance up to 409,000 miles which was well beyond the distance of the Moon. They finally lost contact, not because of signal attenuation due to range, but simply because the batteries went dead. So you can imagine that with more power and with high gain antennas, radioing even across the vast distances of the solar system is entirely within existing technological capability.

Figure 3 shows what we plan to do with our next Juno II. It will not be a lunar probe, but a rather sophisticated scientific Earth satellite. We have a contract from NASA to fire quite a number of additional Juno II's for such and similar purposes. When you try to accommodate several parties in one satellite, you are really up against a problem of communications, and I mean a problem of communications between people right here on the ground. For everyone would like to have a different orbit, everyone would like to draw most of your electrical power supply, everyone would like to have all available telemeter channels, and tie up all the ground stations. So in a way you are better off if you build more smaller satellites and give each party its own vehicle.

Nevertheless, in this next Juno II satellite, which is slated to be fired in June, we have managed to accommodate seven experiments of which two are minor ones. One is, again, for Van Allen; a second concerns itself with the Heavy Primaries in Cosmic radiation; a third one is a solar x-ray experiment.

The fourth is an experiment that had been suggested by Dr. Harry Wexler of the U. S. Weather Bureau. It deals with the distribution of the albedo of the Earth. By that I mean the experiment is designed to measure the energy reflected back into outer space by the Earth and its atmosphere as a function of the cloud coverage underneath the satellite's path. (The measuring gauges consist, essentially, of one white, one black and one special-coated hollow sphere. The temperature difference between these three spheres can be evaluated in terms of the amount of Earth-reflected energy. There will be differences in Earth-reflected energy between areas covered by overcast and other areas where the terrain underneath is cloudless. Further differences result from the latitudinal changes of the impingement angle of the Sun rays. Meteorologists believe that better knowledge of the ever-changing cloud coverage of the Earth, and the resulting geographic pattern according to which solar energy is absorbed by the Earth is one of the most important elements of knowledge we need in order to get a bit closer to their ultimate objective, namely the conversion of weather forecasting from a rather dubious activity to an exact science.)

Other experiments on the satellite involve the so-called "Lyman-alpha" line in the ultra-violet portion of the hydrogen spectrum. Further on, there is a meteor erosion gauge. Solar cells which you see here are used to charge a set of chemical batteries inside the satellite. Solar batteries have already been successfully used in the Vanguard I satellite, but in our case we use the solar batteries to recharge a set of nickel cadmium batteries so that we have continuous uninterrupted radio communication even while the satellite is in the shadow of the Earth. In other words, the solar batteries perform exactly the same function as the generator in your automobile; they keep recharging the battery, so that all the electrical equipment simply draws on the main bus bar and doesn't care whether it gets the energy from the solar batteries direct or from the chemical batteries while the latter are not being charged because the satellite is in the shadow of the Earth.

We have a 20 megacycle, and a 108 megacycle radio transmitter in the satellite. The whole unit weighs approximately 90 pounds.

All our forthcoming Juno II's will be used for satellites and none for lunar or planetary space probes any more.

Figure 4. Our next big ambitious project at ABMA in the space area is the development of a brand new and rather gigantic space carrier rocket that we call the Saturn - simply because for us, like for the Solar System, it came next after Jupiter.

Saturn's first stage, which is presently under development as an in-house project at ABMA is powered by eight rocket engines which are similar to the standard Rocketdyne engine used on the Jupiter. Saturn has a combined take-off thrust of 1 and 1/2 million pounds. Each engine has its own turbo-pump but all engines feed on the same tanks. The tanks are clustered. There are eight tanks of 70" diameter surrounding one central tank of 105" diameter. The central tank is filled with liquid oxygen and so are four of the outer 70" tanks. The other four of the outer tanks are filled with jet propulsion fuel, which is something like kerosene. The tanks are interconnected so that any engine can draw its propellants out of all tanks. In other words, we do not assign propellant tanks to individual engines, but all engines feed out of all tanks. This was very important because ARPA, with whom we have the contract, insisted from the outset that we provide an engine-out capability on this booster. Their philosophy, which is completely identical with ours, runs something like this: You can make a very good point for reliability of multi-engine aircraft as long as that multi-engine airplane can keep flying even with one engine out. However, when a multi-engine aircraft is so designed that it must land if one engine goes out, reliability-wise you would be better off with a single engine aircraft which has fewer parts that can go wrong. But like in aircraft, you can really improve overall reliability of large rockets by going to the multi-engine principle, if only you provide engine-out flight capability. Now this Saturn booster, when carrying two top stages, would lose only very little of its orbital payload capability if one of the eight engines went out. If it has to fly on seven engines, the burning time will be extended a bit, but we still get the same total impulse out of the booster. It just fights against gravity a little longer and it carries the dead weight of the idle engine along, but that is the only performance penalty we have to pay.

This Saturn booster is approximately 82 feet long and will be used as the first stage of a three-stage vehicle. The second stage of the booster will be a modified ICBM, in all likelihood a modified first stage of a Titan. The third stage will be a modified Centaur, which is presently under development at Convair and Pratt & Whitney. The combined three-stage Saturn vehicle has an orbital capability in a 300 mile orbit in excess of 30,000 pounds. It can escape the Earth's gravitational field with a net payload (not including the third stage) of approximately 7000 pounds, and it could place about 2000 pounds of net payload in a soft landing on the surface of the Moon.

Although at this time we have no plans or assignment to do anything like this, Saturn also has a capability of carrying a crew of two or three people around the Moon and bringing them back tangentially into the Earth's atmosphere at parabolic or slightly hyperbolic speed - approximately 7 miles per second. What happens thereafter will probably depend on you people. Because if we have to provide enough propellants in the rocket for a power retardation maneuver to slow that parabolic return speed down to circular orbital speed before re-entry, most of our available payload will be used up for these retro-propellants. However, if aerodynamic re-entry at parabolic speed could be provided (and we think there are ways and means of doing this) then Saturn could really carry a crew around the Moon and bring it safely back to Earth without any fuel consuming power retardation, except perhaps what little trajectory positioning control thrusts may be necessary prior to the actual re-entry into the atmosphere.

This Saturn booster is not a pipedream. It is an approved project, although I could not honestly say it is well financed. The funds certainly are not sufficient to proceed at top development speed but they permit us at least to keep moving. We have a full scale mock-up of the tail section of the first stage down in Huntsville. We have taken delivery of the first engines for the first stage, and we are presently converting our big captive test tower at Huntsville to the static testing program of the entire eight-engine booster system. We hope that by early 1960 the full eight-engine booster will have completed its first static run. The first flight tests are scheduled for 1961.

Figure 5. One application that ARPA has in mind for Saturn is the establishment of a communication satellite system. This satellite system would probably consist of three satellites, each of which is to be brought into a 24-hour orbit with the help of one three-stage Saturn. A 24-hour orbit is an orbit 19,400 nautical miles above the surface of the Earth. In our case it is placed in the equatorial plane and the satellites travel from West to East. As a result, the three satellites, which will be spaced 120 degrees apart in the same orbit, will be stationary over three distinct points on the equator, say, one over the Atlantic, one over the Pacific, and one over the Indian Ocean. It is easy to see how they can be used for transoceanic communications.

This is Japan. Over here you can see the outlines of the North American continent. This is Europe, and this is Soviet Russia, and here again Japan. A person who wants to make a telephone call from Tokyo to London beams his message up to the Pacific Satellite which hangs stationary in the Tokyo sky. Inasmuch as the satellite orbits in the equatorial plane, and since Tokyo is not located on the equator, the satellite will not be directly overhead, but will be stationary under a certain angle of elevation above the Tokyo horizon. The Tokyo telephone center which is equipped with a directional antenna knows, of course, under which azimuth and elevation the Pacific satellite is seen from Tokyo, so that is the direction in which it beams up the call. The satellite, which will look a little bit like the one shown in this figure, will receive the message with one of its dish antennae and will radiate it out again over another dish. This second dish is aimed at another satellite equally equipped, in our case the one stationary over the Atlantic. The latter one then beams the message down to the London telephone center. In this fashion, via the Pacific and the Atlantic satellite, a person in Tokyo can talk to a person in London. Of course, if the two parties are closer together only one satellite is needed. For example, a man living in Washington, D. C., wants to talk to someone in London. Well, the Atlantic satellite is in line-of-sight contact of both Washington and London. So they don't have to bother with the Pacific one at all, but just use the Atlantic satellite here as a single relay station.

We call this scheme "Large Area Coverage" because in this scheme radio dishes with an opening angle exceeding the angle subtended by the Earth would be used. Thus the entire visible Earth as seen from one particular satellite would be enveloped by the beamwidth of the satellite's transmitting and receiving antennae. Any suitably equipped ground station in this whole area can conceivably contact "its" satellite, and can also receive the satellite's messages. Privacy is obtained by use of separate carrier frequencies. Such a system, provided it has enough traffic handling capacity, would therefore be ideally suited for a global telephone, telegraph, and television network servicing the entire Earth. All points on Earth except small areas around the North Pole and the South Pole (where the equatorial satellites would be exactly on the horizon) would be in continuous line-of-sight contact with at least one satellite and could therefore telephone, telegraph and televise wherever on the Earth they want, in direct line of contact.

In the military area it means that an airplane flying anywhere on Earth, or a ship on any of the seven seas, by using the satellite as a communications relay, can now get a message from Washington or from headquarters, or even from the other side of the globe at any time. The contact is possible around the clock and on real time - without any delays such as encountered by the use of so-called "messenger-type" satellites, which employ tape storage and play their messages back as they pass over different countries. All three services should be interested in this kind of communications system.

Figure 6 shows how one can use this same 24-hour communications satellite system for pinpoint coverage. In military communications coding alone may sometimes not be enough and it may be desired to exclude certain areas from any chance of intercepting a message. In this case we would want to use narrow-beam antennas in the satellites that can be focused upon given points on the ground. For example, Washington wants to talk to our Headquarters in Korea, but wants to make sure that the Soviet Union isn't listening in from nearby Vladivostok. In such a case one would want to beam the message down to a very limited reception area. With this scheme it is necessary to provide the satellite with suitable command links and servo drives so one can turn the directional antenna of the satellite transmitter by ground command to any desired point. In other words, it is not necessary to have as many satellite directional antennas as you have ground communicants. This may be important for commercial communications also. Say, for instance, you have a classified message from New York for a customer in Capetown, and for Capetown the commercial Atlantic satellite has no permanent directional antenna. So New York sends a radio instruction up to the satellite antenna controls. Through the radio command link the little servo motors are controlled that turn the antenna. All the New York operator actually has to do is dial Capetown for the antenna setting and computers and automated machinery do the rest. Thus New York sends the message, and when they are through they can turn that same directional antenna at Rio de Janeiro or London. In this fashion it is entirely possible to combine secure pinpoint coverage with global service.

There is an old and general rule in the communications field that with the same expenditure and weight of the equipment used you always have a choice between number of messages conveyed and the quality and the degree of protection of the messages. By and large to protect or secure a message, you will need more power and more bandwidth. The same goes for a higher quality in the transmission.

Why is this communications satellite system so important for the U. S.? Well, I think very few people realize how limited the capacity of our Transatlantic cable system is. If we want to expand this service we'd have to put in more sea cables, and that is an awfully expensive proposition. Of course, there are still the low frequencies that follow the curvature of the earth, but the frequency spectrum in this region is so completely saturated that it can't carry any additional service. Finally, there is the tropospheric and ionospheric scatter technique with short waves which the international radio hams use, but it is utterly dependent on weather and sunspots and other things and simply too unreliable for many vital services. Now the statisticians have found that the number of telephone or radio messages carried over the Transatlantic trunk lines has increased by a factor of ten during the last seven years. If this trend continues, and all indications are that it will, we will really have to do something about expanding our transoceanic communications, and this 24-hour satellite system appears to be by far the most economical and attractive approach. It offers uninterrupted, weather-independent line-of-sight communication by centimeter-band carriers where the supply of frequencies is practically unlimited. Moreover, due to the shortness of the wave lengths used, you get a pretty wide inherent bandwidth which permits transmission of high quality television and high-fi music and voice over plenty of channels.

From the economical point of view, it doesn't take much imagination to see that here we may have at least one application of space flight with a very good chance of paying for itself. Just imagine, we could charge a fellow a penny a word for this service. We'd sure be able to finance all our future expeditions to Mars and Venus out of this revenue!

Figure 7. The same Saturn vehicle you just saw could, of course, carry payloads not only into 24-hour orbits, but also to the surface of the Moon. As I said before, we could land approximately 2000 pounds of net payload softly on the surface of the Moon. On this figure you see a lunar soft-landing vehicle which represents a typical design for a Saturn-booster top stage designed for a soft landing on the Moon. Let me hurry to add again that this is not an approved project but merely the result of the many feasibility studies we are conducting about Saturn applications, and believe me,

it turns out that many things are feasible with a space rocket as powerful as Saturn!

If you want to land an unmanned package on the Moon consisting of a highly intelligent little automatic laboratory which is designed to give us some information about the surface environment of the Moon - a little automatic telemeter radio station with all kinds of end organs to tell us about the hardness and the radioactivity of the lunar soil or the depth and composition of the lunar dust and all these kinds of things - then you must be sure that your retardation rocket does not change the environment you want to investigate. Specifically, it is mandatory that the retardation rocket does not blow the lunar dust away in the landing area. Now here is what we are proposing to do. The figure shows the landing vehicle with its rocket engine, fuel and oxidizer tanks, pressurizing bottles and, of course, with its pretty elaborate guidance system. Shortly before the end of the rocket-retarded descent, at an altitude of, say, 100 feet above the lunar surface, we simply detach the retardation rocket from the payload proper, and let the payload drop to the surface of the Moon. We can easily afford to do that because of the weak lunar gravity which doesn't cause the buildup of very high speeds in such a 100-foot drop. The moment we detach the payload, the retardation rocket, being reduced in weight, will start rising again. With a guidance-induced tilting program we can make it fall a couple of miles away from the impact site of the payload package itself. The payload unit, upon impact, will deploy its various end organs and antennae, turn on its radio gear and, if everything works well, will be ready to send messages back to Earth. With the Saturn the equipment that could be landed in this fashion on the Moon would weigh about 2000 pounds. By use of modern microminiaturization techniques, quite a little bit of sophistication can be put into such a 2000-pound package to tell us what it's like on the Moon.

Figure 8, please. Now the next step in our endeavor in Space flight (I'm not talking any more solely about our Army Agency in Huntsville, but about our National Space Program) will be, of course, to put people on the surface of the Moon. Here we are faced with a rather fundamental question: Shall we do that in a direct flight from the surface of the Earth to the surface of the Moon and back, or shall we provide orbital refueling for the vehicle that carries the crew to the Moon and back? This figure is a result of a study that we have made to compare the two methods. In both cases we have assumed that two men shall be placed on the Moon and that these two men, after a soft landing on the Moon, have a return capability to the Earth without assistance from an additional vehicle. We have also assumed that no propellants other than for minor path corrections are necessary after departure from the Moon to retard the vehicle again as it tangentially approaches the Earth. The task of gradually reducing the parabolic or slightly hyperbolic approach velocity would rather be performed by aerodynamic drag alone. The question of whether or not this is possible, I should leave with you people here. In other words, can the capsule technique which NASA uses for the Mercury program at orbit-return velocities of 18,000 miles per hour still be used at velocities of 25,000 miles per hour such as occur during return from the Moon? The vehicle picks up this tremendous additional speed, of course, during its free fall through the gravitational field of the Earth.

Well, as far as the two top stages are concerned, the same vehicle capable of retro-landing on the Moon and departure therefrom has been assumed in both cases of our comparison (Figure 10). In the first case, however, we have determined what kind of a set of booster stages it takes to impart a sufficient initial speed to this lunar landing vehicle to get it to the Moon in a direct ascent from the surface of the Earth. On the left side of Figure 8 you can see the dimensions of the first four stages required to produce the necessary speed to cross the neutral point between the Earth and the Moon's gravitational field. The fifth stage is made up by that part of the lunar landing vehicle needed for the retardation during the letdown on the Moon - in other words, it comprises the propulsion system needed to reduce the velocity of impact to zero so that a soft landing is possible. The sixth stage finally is necessary to break away from the Moon again and return to the Earth's gravitational field.

In the second case we have assumed orbital refueling (right side, Figure 8). Thus, for the take-off from the Earth we need only a vehicle capable of hauling propellants into a low orbit. The same three-stage rocket can also fly an empty lunar landing vehicle into the same low altitude orbit. It will arrive there together with an empty third stage of the three-stage carrier rocket. We have now assumed that the propellant carried up in the payload compartments of these five orbital "tanker" rockets is transferred into the exhausted tankage of the third stage of the vehicle that carried the lunar landing vehicle itself into orbit, which is still attached to it. Refueling of the third stage and also the fourth (Moon-landing) stage completed, this vehicle will then depart from its low orbit to the surface of the Moon. It will use the tankage of the former third stage to acquire the velocity necessary to increase its orbital speed to that of escape speed. The third stage will then be detached and discarded. The lunar landing vehicle will then land on the surface of the Moon on its spiderlike legs, using the fourth stage tankage for the retardation maneuver. With the fifth stage propulsion system the crew will finally break away from the Moon again and return into the gravitational field of the Earth. Remember, it doesn't take very much speed to overcome the Moon's gravitational field and to fall back into the gravitational field of the Earth - any V-2 could do it.

The result of the study is apparent. If you choose a direct transfer method, you need a huge vehicle of 438 feet length and 12 million pounds of thrust, whereas in the case of orbital refueling the job can be done with vehicles employing, not exceeding size and weight of our Saturn. To be sure, it would require somewhat more sophisticated second and third Saturn stages using hydrogen-oxygen, but basically it would still be a Saturn. So we see that by adopting the orbital refueling technique, it is possible to launch such an expedition to the Moon with vehicles which are already under active development. We do not have to wait until a futuristic monster rocket of 12 million pounds of thrust becomes available.

Figure 9 shows how such an orbital refueling operation would be carried out. At the bottom we see the third stage of the tanker rocket after arrival in the orbit, with the nose tankage full of propellant payload. Floating above and behind it we see the vehicle which after refueling will proceed to the Moon. It has arrived in the orbit in an ascent from the Earth's surface using the same kind of first and second stages that are also used for the tanker rockets. The first and second stages, of course, have been discarded prior to attainment of the orbit, and upon arrival in orbit the third stage tankage is also exhausted. During the orbital refueling operation the tanker's propellant payload is now transferred through this umbilical feed line into the empty third-stage and fourth-stage tanks of the Moon vehicle. It takes five tanker flights to refill the Moon vehicle's tanks. But after refueling it is ready to carry its two occupants all the way to the surface of the Moon and back to the surface of the Earth. The large rear stage of the craft (the former third stage) provides the velocity increment necessary to transfer from the low departure orbit into the hyperbolic trajectory to the Moon. After accomplishing this, it will be discarded.

Figure 10 shows the two top stages of the Moon vehicle. This is the portion that actually lands on the Moon. Propellant in the lower section is consumed to retard it on the Moon. We can see the two retro-rocket engines and the landing gear. On top of the retardation stage you see the final rocket stage that is needed to break away from the Moon after the ground exploration has been completed. Finally, in the tip of the craft, in inverted position, we can see the nose cone which will provide re-entry back into the Earth's atmosphere.

Figure 11 shows only the front portion of the lunar landing vehicle, which for the sake of clarity is depicted once more in its entity in the boxed insert. We see a man climbing up to his contour chair in the nose cone - where his companion is waiting already.

Let me say a few final words about this "back-from-the-Moon" nose cone. It must be capable of hyperbolic re-entry into the atmosphere. This means that upon tangential entry into the atmosphere at a speed slightly in excess of escape velocity, it must first slow down to circular orbital speed, and then on through the entire realm of hypersonic and supersonic speeds down to sound speed and subsonic speed. At the end, we would deploy the good old parachute, and the Navy would stand by to fish our two heroes out of the water.

There are two fundamental problems which make such hyperbolic re-entries quite a bit trickier than Mercury-type re-entries from near-circular orbits. One problem, of course, is posed by the greatly increased rates of heat transfer, but we are quite confident that we can solve it with existing techniques. The other difficulty lies in the fact that a body approaching the Earth tangentially at hyperbolic speed will tend to sweep around the Earth and (after passing through the lowest point, or apex of the hyperbola) leave the Earth again on the hyperbola's "escape leg". This can be prevented only if, during the first pass through the atmosphere, the body's velocity is retarded to sub-escape speed. It will then enter an ellipse around the Earth, the perigee of which will again pass through the atmosphere, allowing further retardation. In principle, it appears possible to slow the body down in a successive series of "braking ellipses" of ever-decreasing apogeal altitudes. But this procedure is hardly compatible with the stringent requirements for terminal navigation, -- after all, the occupants must retain a degree of control over the question in which of the Seven Seas they are going to land.

Therefore, it appears mandatory to execute the entire landing operation from hyperbolic re-entry to touchdown in one uninterrupted maneuver. This means that in the vicinity of the apex of the approach hyperbola sufficient negative (i.e., downward) aerodynamic lift must be generated to prevent the body from climbing out to higher altitudes again. A simple calculation shows that in order to convert the hyperbolic path into a circular path around the Earth, a centripetal acceleration of almost exactly one "g" must be produced by this downward-directed aerodynamic lift.

It may be difficult to do that with a body of rotation such as a Mercury type nose cone. On the other hand, glider-type configurations may run into serious difficulties on account of the high total heat transfers to which they would be subjected as a result of their long flight times through the atmosphere. Maybe some kind of an unsymmetrical nose cone with high drag and variable lift (whose angle-of-attack can be controlled by internal weight-shifting or by small external flaps) is a better solution.

It is to you here at the Naval Ordnance Laboratory that we look for further guidance in this most intriguing field of hyperbolic re-entry and return.

Thank you.

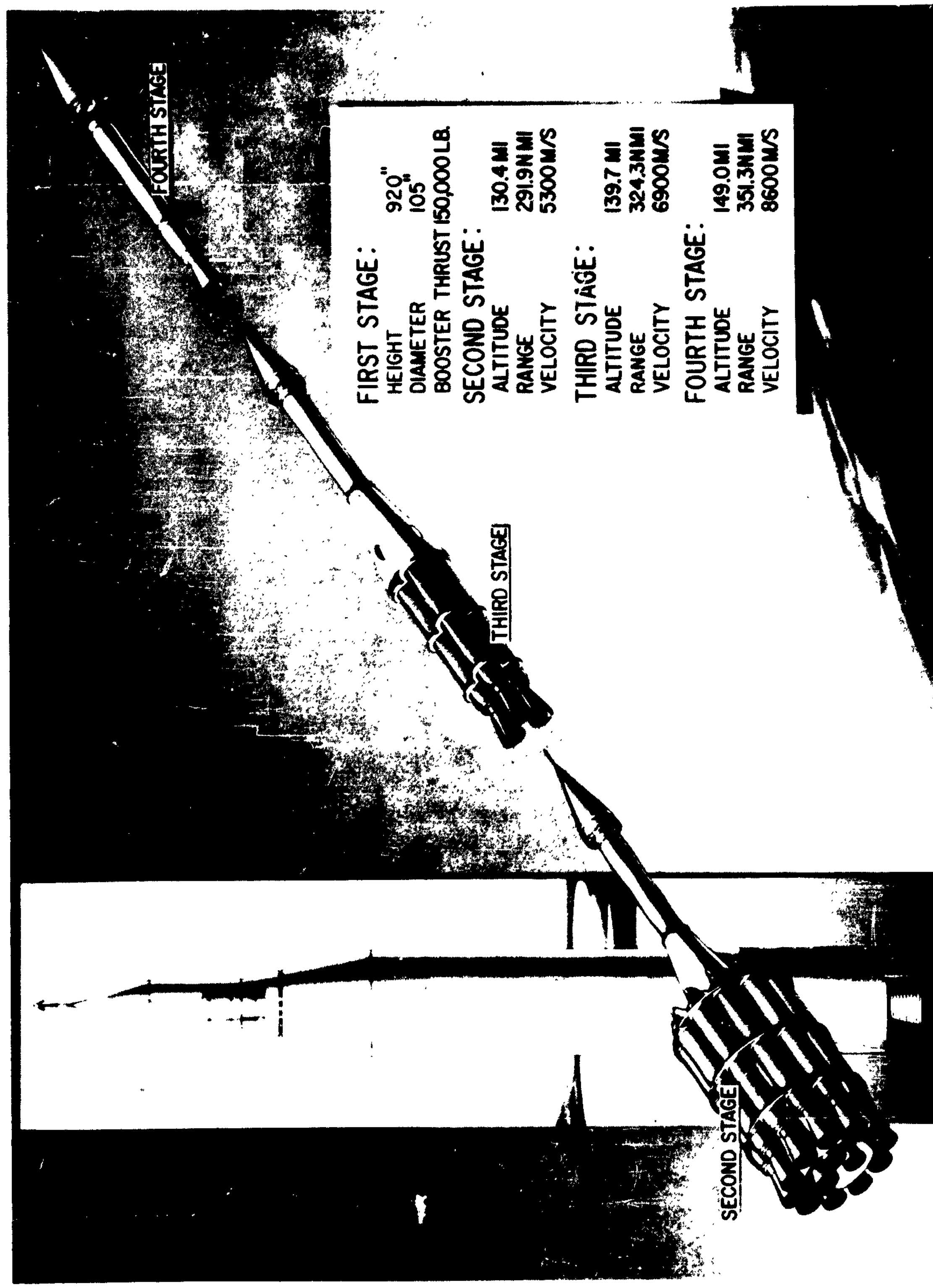


Fig. 1 JUNO II Missile

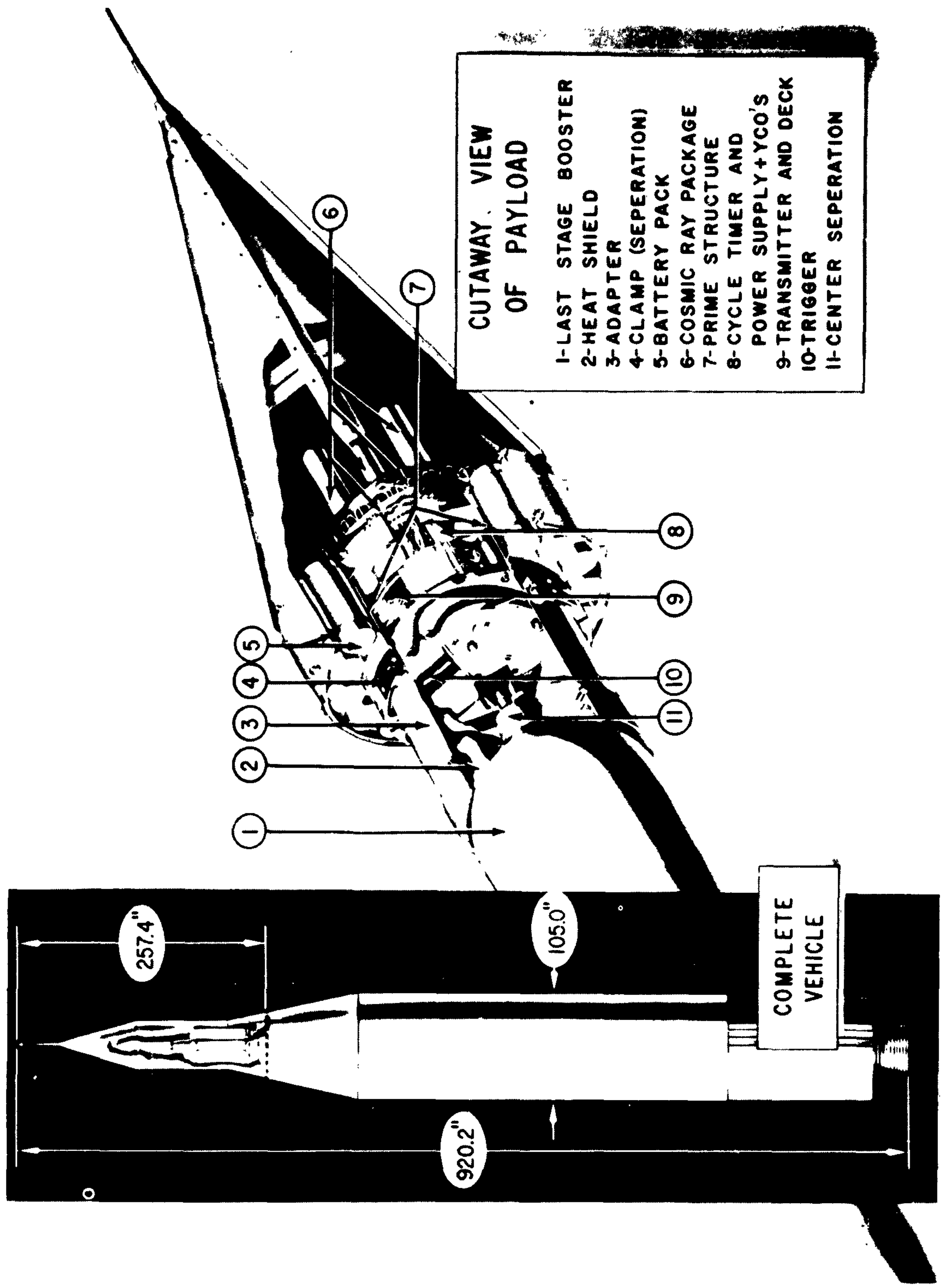


Fig. 2 Cutaway View Of Payload, JUNO II

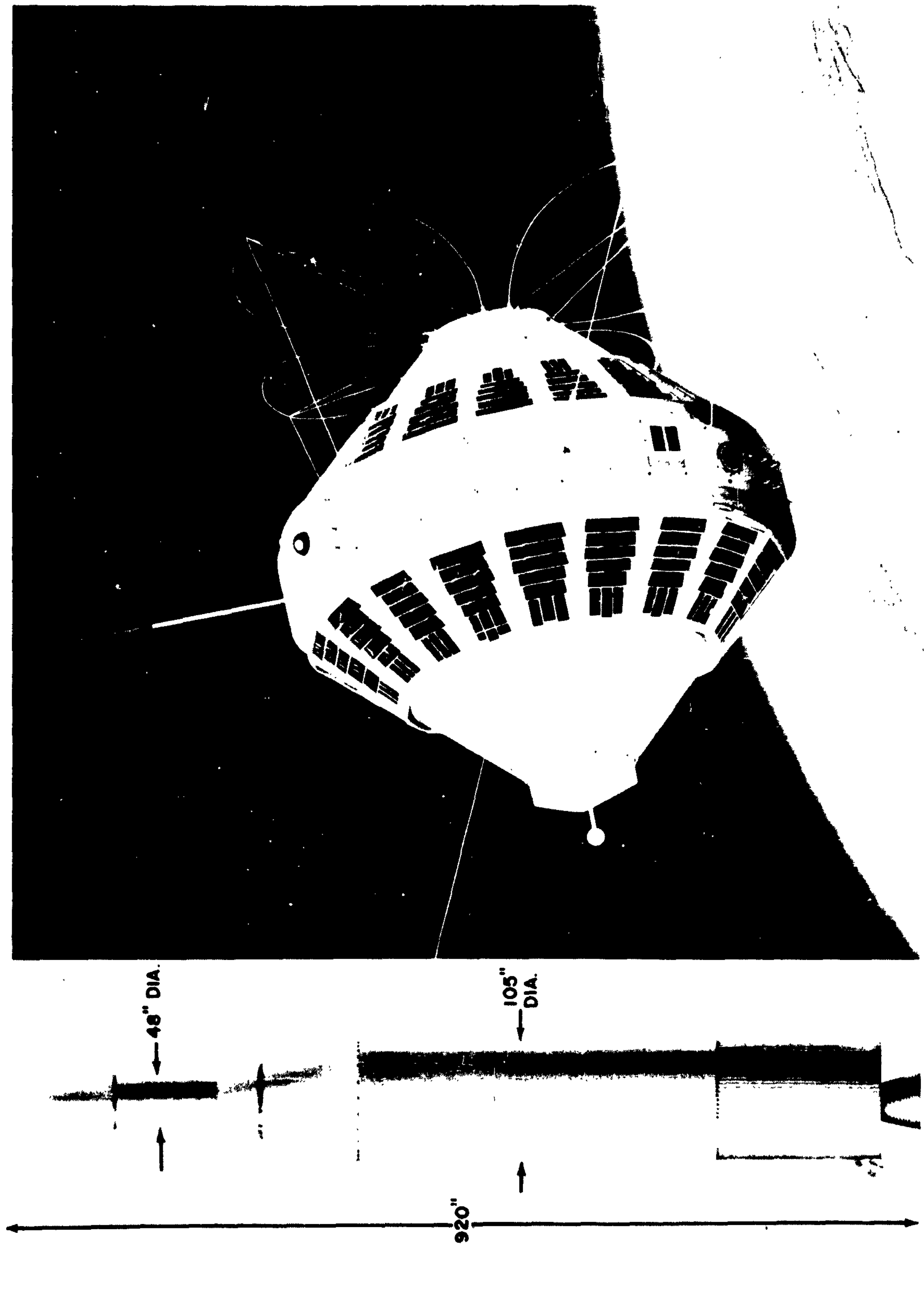


Fig. 3 JUNO II Satellite

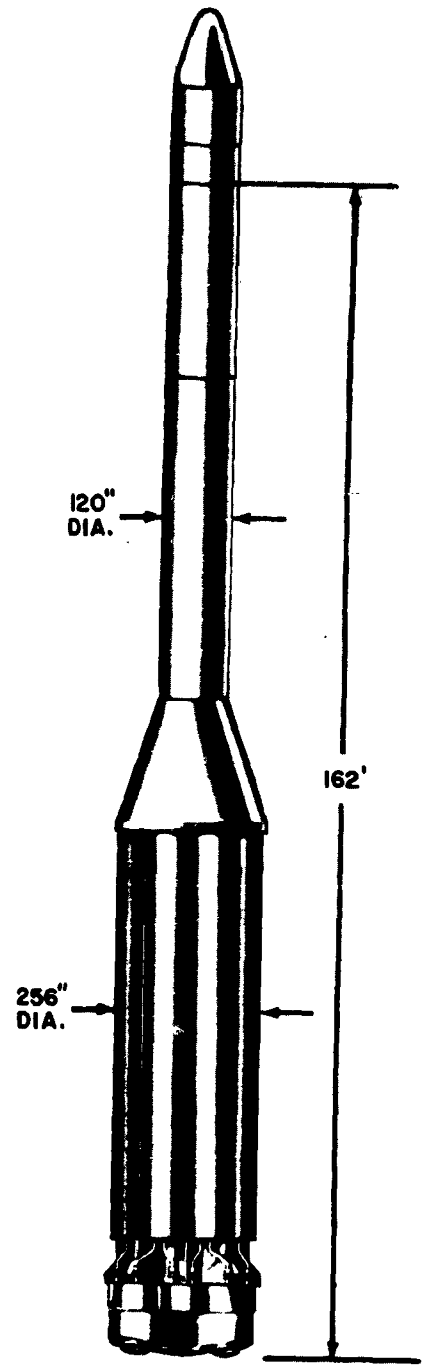


Fig. 4 Three-Stage SATURN Vehicle

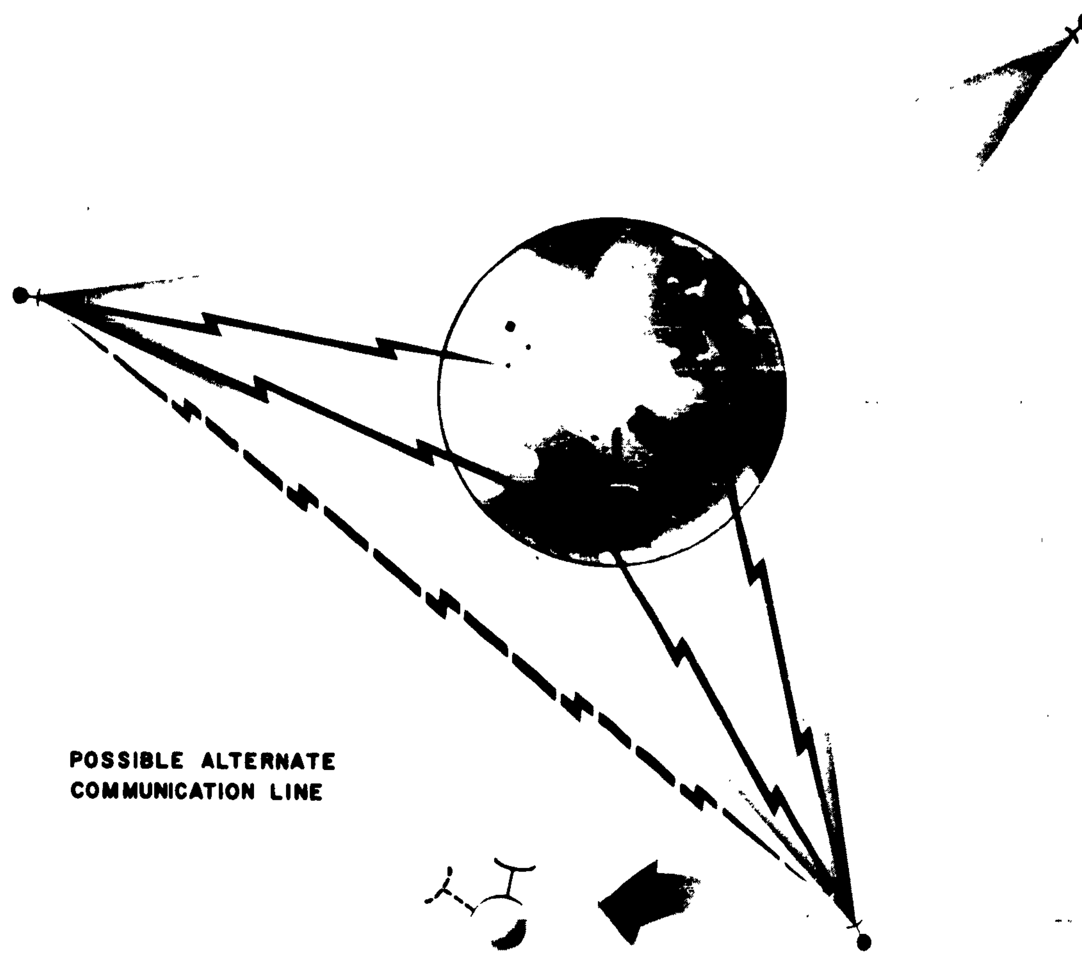


Fig. 5 Satellite Communication System, Large Area Coverage

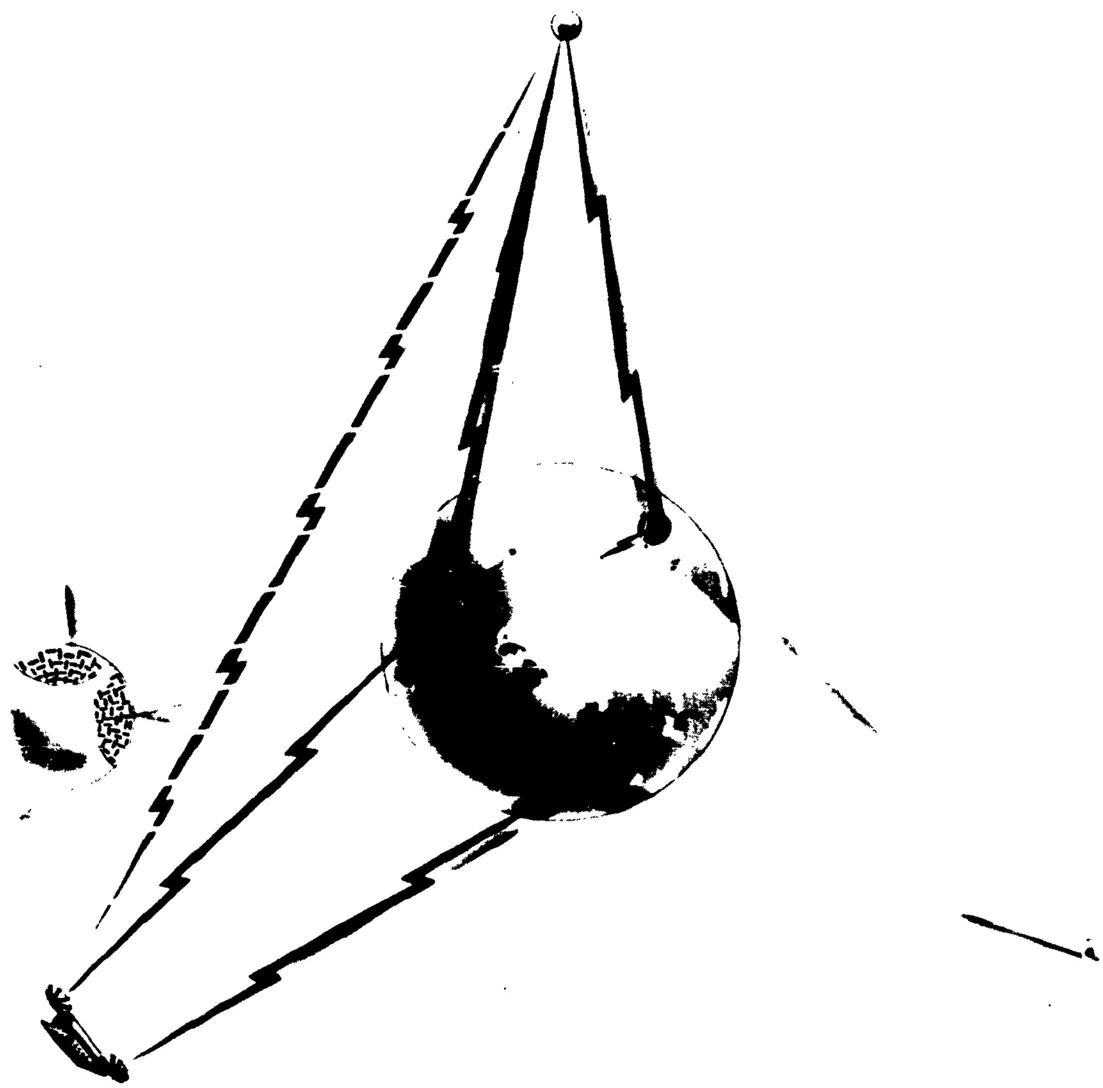


Fig. 6 Satellite Communication System, Pin Point Coverage

Fig. 7 LUNAR Soft Landing Vehicle

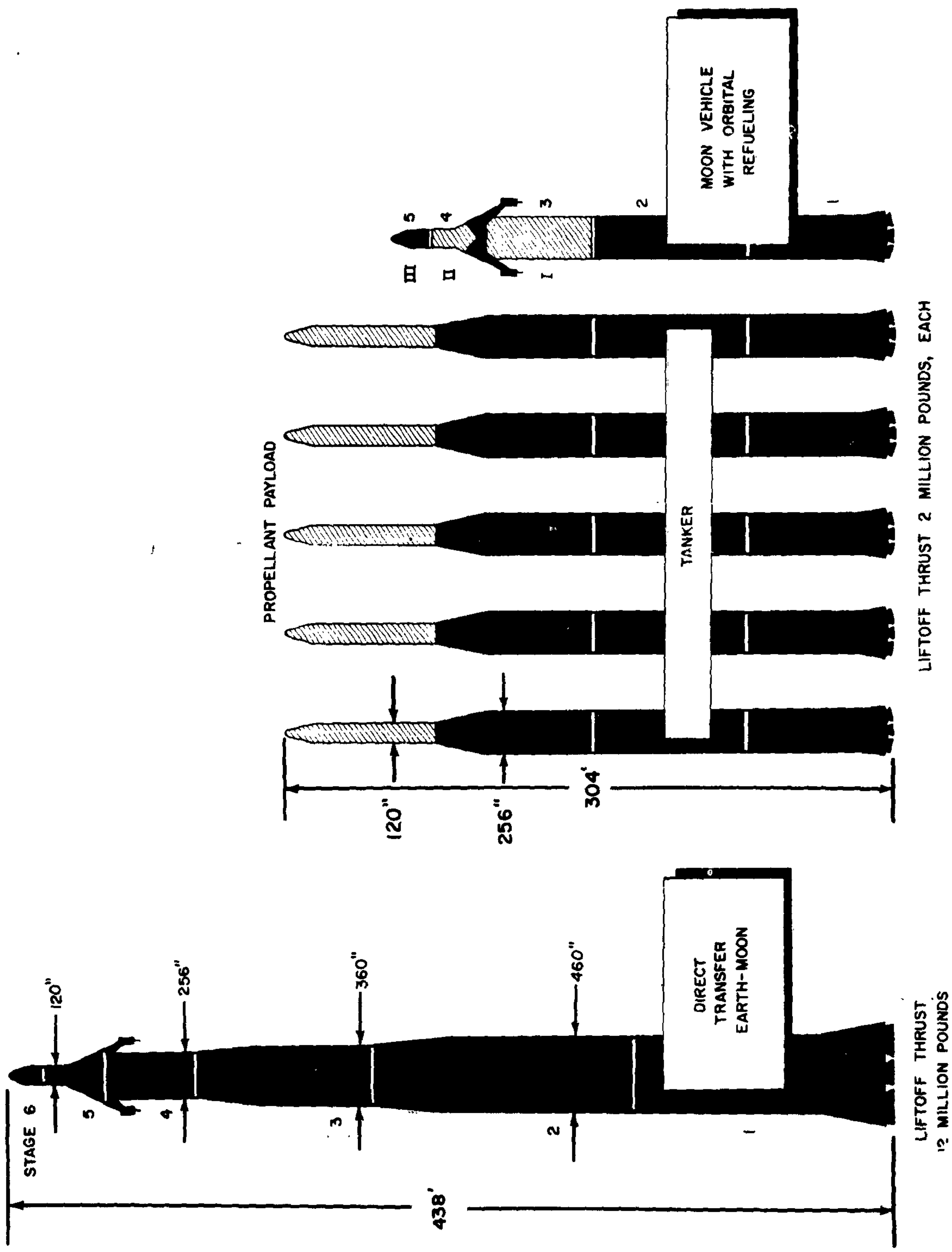


Fig. 8 2-Man Round Trip To LUNAR Surface

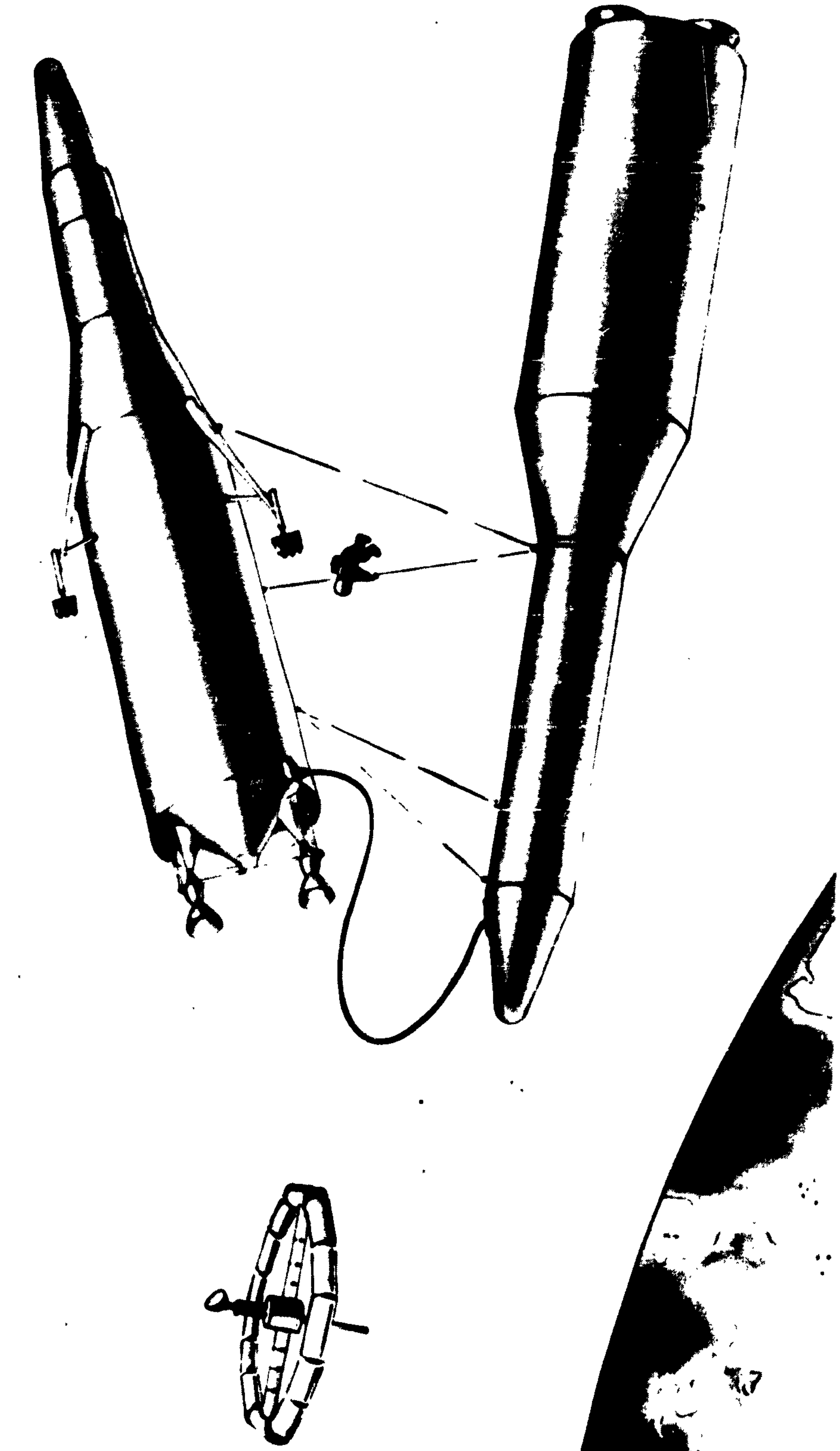


Fig. 9 Fueling Of Orbit-Launched Space Vehicle

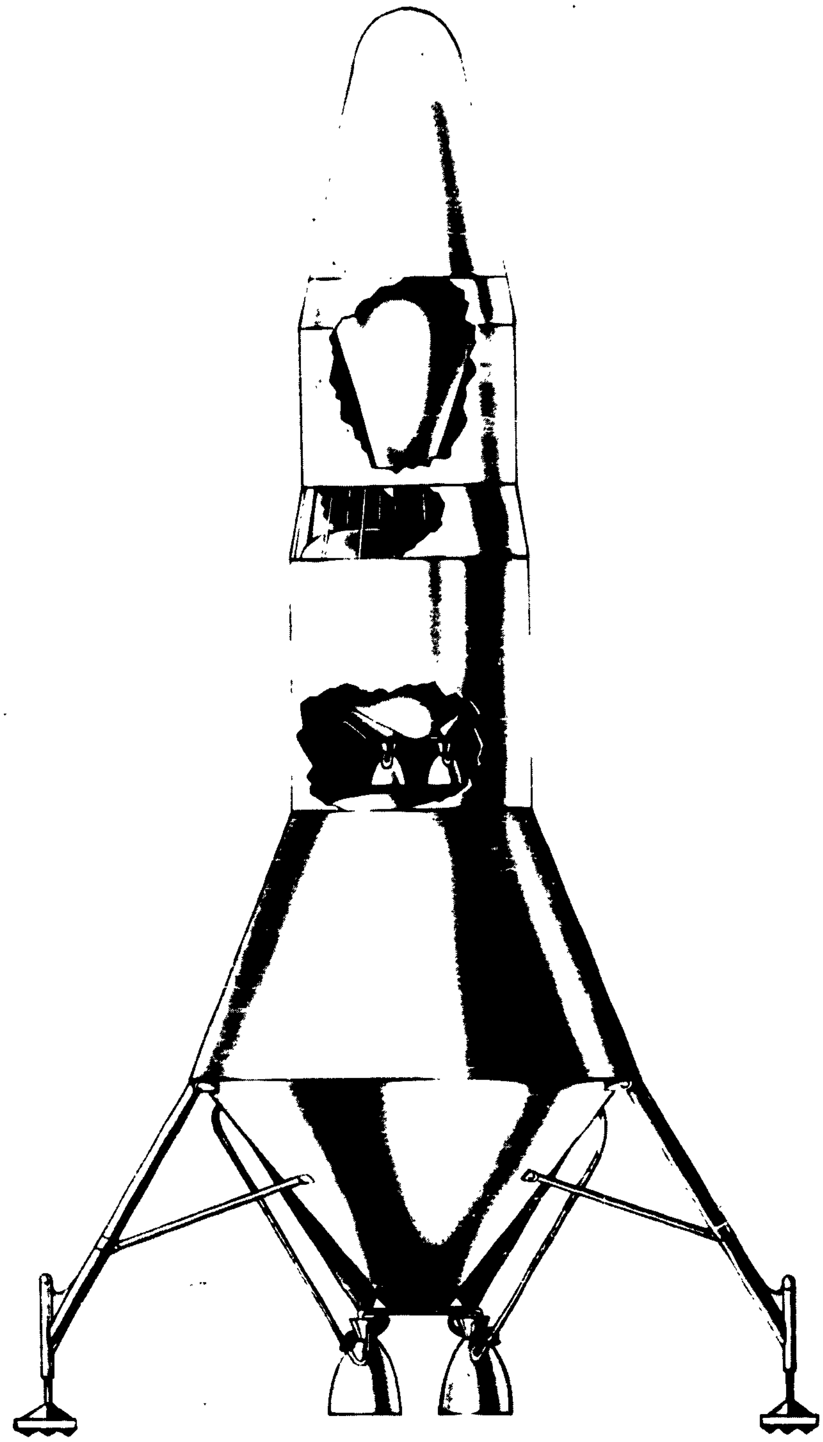


Fig. 10 LUNAR Landing Vehicle

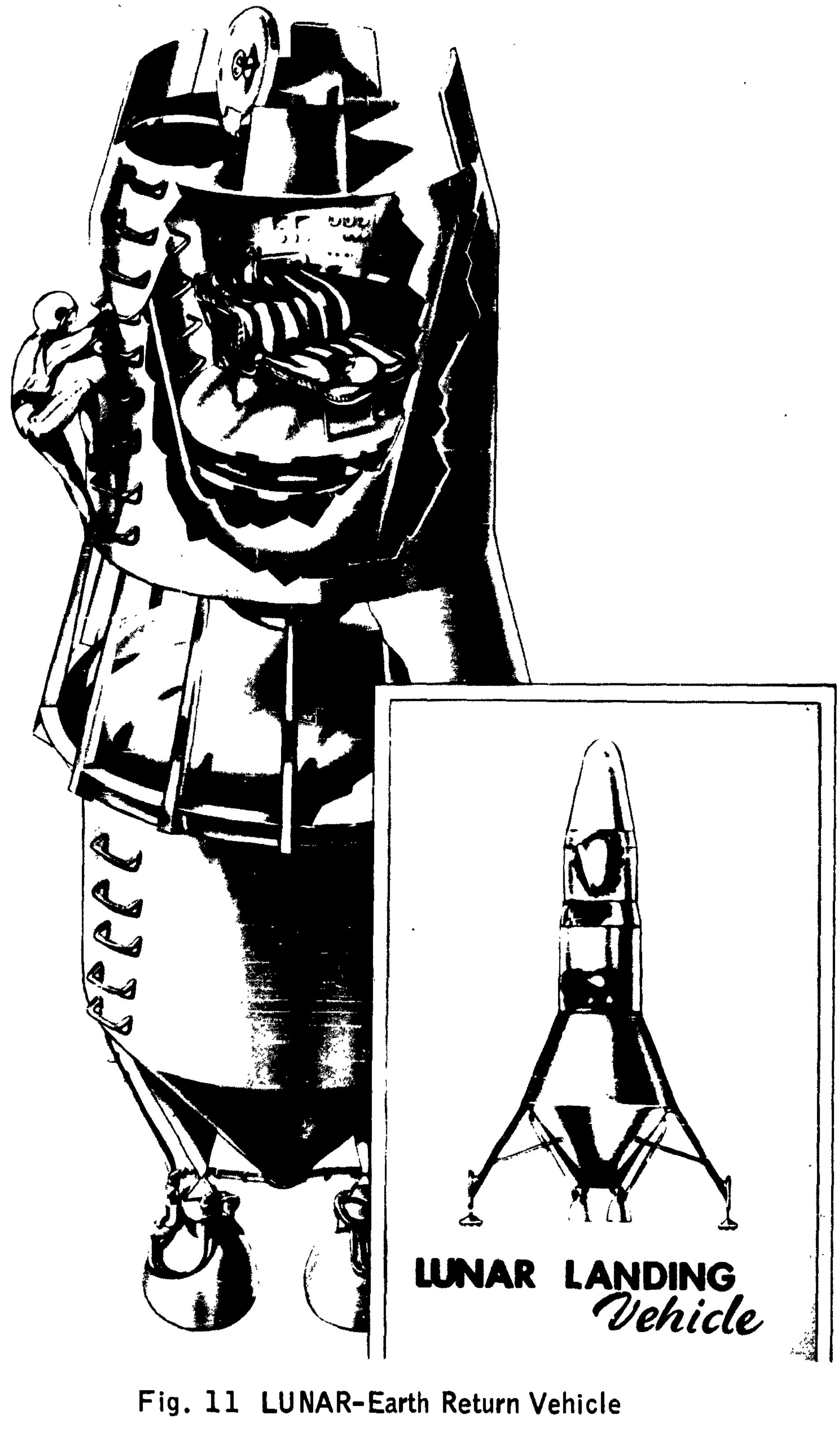


Fig. 11 LUNAR-Earth Return Vehicle

COMMENTS ON PHENOMENA OF HIGH-SPEED IMPACT

by
A. C. Charters*
Chief, Hypervelocity Ballistic Range Branch
National Aeronautics and Space Administration
Ames Research Center, Moffett Field, Calif.

Introduction

Some years ago, when aeronautical engineers were crashing the sound barrier, it was fashionable to begin a paper on aeronautics with the phrase "With the advent of high-speed flight". The sonic barrier has long been surmounted, but the phrase still seems appropriate. The barrier now is the escape velocity from the earth's gravity, and the scope of interest has broadened accordingly. This paper is an example of the departure from the classical discipline of aeronautics that high-speed flight has brought about.

My subject is the phenomena of high-speed impact. The physics of impact is an end in itself, but two practical examples are of importance to space flight: The protection of spacecraft from meteoroids and the physical surface of the moon.

Meteoroids travel at velocities from 35,000 to 240,000 ft/sec. They are small for the most part and relatively scarce -- Whipple estimates that a 3 meter sphere would travel for 3 days before being struck by a 1-mm meteoroid -- but their impact is so devastating that spacecraft will require protection if they are to travel for any length of time.

Selenography -- the lunar equivalent of geography -- is now a subject of practical interest because plans are being laid for a lunar expedition. The moon offers an unfriendly environment at best, and it is prudent to learn as much about its selenography as possible. Despite its proximity, many features of the lunar surface are somewhat of a mystery, and, in particular, the circular formations known generally as "craters". Some people think these craters are formed by the impact of meteoroids. So here, possibly, is another example of high-speed impact.

*This paper was co-authored by:
James L. Summers, Aeronautical Research Scientist

Impact is an old subject to military engineers. Unfortunately, the velocities of artillery shell are too low to explain the impact of meteoroids, because the physics of impact undergoes a fundamental change if the velocity increases beyond a certain value.

In the last few years, experiments have been carried out at velocities and under conditions which do simulate the impact of meteoroids -- that is, the simulation appears to be right, so far as we know. But the data are very sparse, and they conflict with one another to a certain extent. Even so, if one could turn to a reliable theory of high-speed impact, he would be in a fairly comfortable position with only an empirical constant or two to tie down. Alas -- here the confusion is worse, if anything. There is no common agreement on a "physical model" of the impact process, and the predictions of the various theories are widely divergent.

One should keep in mind that impact is a complex physical phenomena. All three states of matter, solid, liquid, and gas, may be involved in the transformations that take place during the impact process, and at high speed there is ample energy for these transformations, should the physics require them. The collision between projectile and target takes place so rapidly that the forces generated may be many times, even orders of magnitude, greater than the strengths of the materials in their solid states. As a result, the deformations may be so large that the solids act as though they were liquids, even though their physical state does not change. Here, we are dealing with the properties of materials that stretch beyond the borderlines of knowledge. The relations between stress and strain at very high rates of strain are not certain. The equations of state of solids subjected to sudden pressures in the megabar region have yet to be established. Furthermore, the energy consumed during impact is extreme. A projectile traveling at 10,000 ft/sec has a kinetic energy per pound equal to TNT; at the minimum velocity of meteoroids, the kinetic energy is an order of magnitude greater than TNT and at the maximum velocity nearly three orders of magnitude greater. In view of these factors, the confusion existing in this field is quite understandable.

Regions of Impact

In their excellent book on selenography "The Moon", Dr. H. Percy Wilkins and Mr. Patrick Moore conclude their introduction with the statement: "Selenography must be founded on observation, not on preconceived and often erroneous conceptions; let us be observers first and theorists afterwards". This is sound advice and, so at this point, I would like to show you the results of some experiments.

Figure 1 shows the results of firing 1/8-inch tungsten carbide spheres into lead targets at various speeds. On the graph, the ordinate is the penetration and the abcissa the velocity. Representative target blocks were sectioned, and photographs of the sections are inserted above the portions of the graph to which they belong.

At low velocities, the sphere is strong enough to resist the forces of impact, and it penetrates the target as an unbroken projectile. The crater in this case is a deep narrow hole. The penetration increases with the 4/3 power of the velocity, which, incidentally, agrees with the well-known DeMarre formula for armor plate penetration and with a NDRC formula for the penetration of concrete.

At a certain velocity, the forces of impact exceed the strength of the sphere, and the sphere is fractured as it penetrates the target. In these tests, the sphere was fractured at a velocity of 1440 ft/sec at which the impact pressure in the lead, $1/2\ \rho V^2$, was 160,000 psi.

As the velocity is increased, the basic character of the impact begins to change. At first, the sphere breaks into a few large pieces; then, it fragments into finer and finer particles. The crater broadens from a narrow hole to a hemispherical cavity. The penetration first increases a little, then falls off slightly, and then increases again.

As the velocity is increased further, the impact again follows definite and predictable laws. This region of high-speed impact has been called the "fluid impact" region, because the sphere and target act as though they were fluids. Their true physical state for the most part is that of a solid, although there may be a little melting on the surface of the crater. But the forces of impact are so much greater than the strengths of the materials of either the sphere or the target that the sphere and target deform as though they were fluids.

For the case shown in this figure, the fluid impact region begins at a velocity of about 7200 ft/sec. The impact pressure in the lead at this velocity is 4,000,000 psi. If the strength of the sphere is measured by the impact pressure at the start of fracture, the impact pressures in the fluid region exceed the strength of the sphere by a factor of 25 or more.

Fluid Impact

The region of greatest interest to high-speed flight is the fluid impact region, and most of the research on impact at the Ames Research Center has been devoted to this region. The results of the Ames experiments could be correlated by empirical formulae; they have also suggested a model of the impact processes. In the spirit of being "observers first and theorists afterwards", I wish to show you the results of our research and then to discuss briefly the physical model which they suggest.

The experiments consisted of firing small spheres into massive targets. The spheres were about 1/8-inch in diameter and were metal, covering a range in density from magnesium alloy to tungsten. The targets were plates of lead and copper and were massive compared to the craters. The velocities ranged from a few thousand to 12,000 ft/sec.

Figure 2 shows the crater volumes. The measured volumes of the craters, divided by the volumes of the spheres, are plotted as ordinates. The abcissa is a parameter composed of two factors. The first is the ratio of the density of the sphere to the density of the target. The second is the ratio of the impact velocity to the speed of sound in the target, called the "impact Mach number" by J. H. Huth, who first suggested using this ratio. The speed of sound used in reducing these data is the so-called "bar velocity", equal to 4025 ft/sec in lead and to 11,670 ft/sec in copper. It can be seen that the volume ratios are correlated fairly well by a parameter equal to the square root of the density ratio multiplied by the product of the density ratio and the square of the velocity ratio, thereby supporting the contention that the volume of the crater is proportional to the kinetic energy of the projectile. In fact, you can see that multiplying both sides of the equation by the projectile volume makes the numerator inside the brackets equal to twice the kinetic energy of the projectile. The denominator inside the brackets is equal to the modulus of elasticity of the target material. Thus, the crater volume is proportional to the kinetic energy of the projectile divided by the modulus of elasticity of the target material, this ratio multiplied by the square root of the density ratio. It is the addition of this factor, the square root of the density ratio, that is the new contribution to the familiar crater volume - projectile kinetic energy equation.

The role played by the density-ratio factor is that of a cavity shape parameter. If the sphere and target are of the same material, the crater is very nearly hemispherical. If the sphere is less dense than the target, the crater has the appearance of a broad but shallow spherical segment. If the sphere is much more dense than the target, the sphere crater resembles half of a prolate spheroid with the penetration deeper than the radius of the crater at the surface.

Figure 3 shows the penetration in diameters as a function of an impact parameter composed of the same factors as before, namely the density and velocity ratios. In this case, however, the abcissa is the product of the two ratios, directly. As you can see, the experimental data are correlated quite well by the 2/3rds power of the impact parameter.

The penetration formula here is consistent with the formula for the crater volumes shown in the previous figure. If the density ratio is unity, the crater shape is very nearly a hemisphere; cubing both sides of the equation, one sees that the volume of the crater is proportional to the kinetic energy of the sphere. Of course, if the density ratio is different from unity, the departure from a hemispherical shape must be taken into account.

The formulae for penetration are derived from firings into lead and copper targets only, but the authors suggest that they may be used to predict the results of impact in targets of any ductile metal. Of course, this is the essence of Huth's hypothesis of an impact Mach number based on the speed of sound in the target material.

Figure 4 shows the data from firings made at the University of Utah. Spheres were fired into targets of the same metal, and the penetration in diameters is plotted against the impact Mach number for the four cases of aluminum into aluminum, tin into tin, zinc into zinc, and (in one graph) copper into copper and lead into lead. The solid lines are the Ames penetration formula; as you can see, it represents the data fairly well. The density ratio is unity in these cases, but it should be noted that the speed of sound in the materials of these targets varies from 4,000 ft/sec for lead to 17,000 ft/sec for aluminum.

Figure 5 shows more data, part from firings of spheres at the University of Utah and part from firings of cylinders at the NRL, Naval Research Laboratory. In these cases, as you can see, the Ames formula does not represent the data too well.

The data differ from the formula in two respects. First, the penetration increases at a rate greater than the 2/3rds power of the velocity. Second, the data are shifted to the right of the curve, suggesting that the speed of sound in the target material does not fully correlate the penetrations into different metals. The authors believe these differences can be accounted for. Since they are due to different causes, they should be discussed separately. Let us consider, first, the rate of increase with velocity.

Impact Transition

The change in penetration as the transition region is traversed could readily account for the discrepancy in the rate of increase with velocity, particularly if one were unaware of the existence of transition and thought that he were dealing with a single region of high-speed impact. Now, you have already seen how the impact of a tungsten carbide sphere into a lead target changes through the transition region, but you should not suppose that these results are typical. Transition is a subtle process, and both its character and its limits vary as the conditions of projectile and target are varied. Figure 6 shows two examples of transition: the first is the familiar case of tungsten carbide spheres and lead targets; the second is the case of copper spheres and copper targets. It is evident that the change in penetration through the transition region is entirely different in the two cases. So, you can see that the penetration could vary with velocity less rapidly or more rapidly than the 2/3rds power, if the impact lies in the transition region.

Model of Fluid Impact

The second shortcoming of the penetration formula is its reliance on the speed of sound to correlate penetrations in all materials. Actually, the speed of sound does work pretty well, but, nevertheless, there has always been some doubt in the authors' minds as to the significance of the sonic velocity as the primary variable in the physics of impact. The speed of sound is an elastic property of the target material and the high rates-of-strain, large deformations, and plastic flow of metal during impact are far from the weak, elastic deformations of ordinary sound waves. Also, the sonic velocity does not appear as the boundary between two regions of impact as it does between the subsonic and supersonic regions of flow in gas dynamics. For example, the firings in lead extend from low subsonic impact Mach numbers to a supersonic impact Mach number of 3, and the penetration marches through the speed of sound in an orderly manner without any of the changes which one would find in a similar excursion through the speed of sound in a gas.

This anomaly between fluid impact and gas dynamics has stimulated the authors to seek another property of the target material as the dominant variable of fluid impact. Their search has not as yet disclosed this elusive property, but their studies have led them to devise a model of the fluid impact process. To be sure, their model falls far short of a proper theory, but it does go a step beyond the purely dimensional analysis which guided Huth in formulating his hypothesis and it shifts the emphases from the speed of sound to the stress-strain properties of the target material. And, for our discussion of the moment, it leads to a revision of the penetration formula and so may shed some light on the problem of correlating impact in any and all ductile metals.

Figure 7 outlines the proposed model of fluid impact. The impact is assumed to take place in two steps, roughly similar to the response of a ballistic pendulum. You will recall that the projectile strikes the pendulum and sets it in motion, the two exchanging momenta; the pendulum then swings back and up, using up its kinetic energy in work against the force of gravity.

In the first step of the model here, the projectile is assumed to flow into the target, as suggested by our experiments and, in so doing, to set into motion a certain amount of the target's material. The material in motion -- part projectile and part target -- is assumed to be contained in a uniform hemispherical shell which is expanding everywhere with the velocity u. In order to simplify the analysis, we will take the simplest case of projectile and target made of the same metal. Next, we will use a relation derived in the hydraulic analogy of shaped charge penetration and will assume $u = 1/2V$, where V is the impact velocity of the projectile. Now, we appeal to our knowledge of the ballistic pendulum and assume that the momentum of the projectile is transferred to the momentum of the fluid shell. At this point we can compute the mass of material in the fluid shell. The result is that

$$m_{\text{FLUID SHELL}} = 2m_{\text{SPHERE}}$$

In the second step it is assumed that the fluid shell expands outwards forming the crater and that its expansion is resisted by a deformation stress in the target material. The stress is assumed to be constant during the formation of the crater -- an assumption introduced for the purpose of simplicity, although it has some justification in that ductile metals deform plastically at roughly constant stress. Again we appeal to our knowledge of the ballistic pendulum and assume that the kinetic energy of the fluid shell is used up by the work of deformation in forming the crater. Taking the results of the first step and carrying out the integrations required by the second, we obtain a relation between the deformation stress and the penetration, namely

$$s = \frac{3}{8\pi} \frac{m_P V^2}{\bar{p}^3}$$

Since the deformation stress is assumed to be constant, this equation tells us that the penetration is proportional to the 2/3rds power of the velocity. Since the crater is assumed to be hemispherical, this equation also tells us that the volume of the crater is proportional to the kinetic energy of the sphere. Consequently, the results of the analysis and the experiment are consistent.

This analysis gives us the variation with velocity only and not with density, since the sphere and target were taken to be the same metal. However, we can use our empirical relation for the variation with density and can revise the formula accordingly. When the density ratio is included, the penetration equation derived from this analysis is the following:

$$\frac{p}{d} = \frac{1}{2}\left(\frac{\rho_P}{\rho_T}\right)^{1/3}\left(\frac{\rho_P \; V^2}{2s}\right)^{1/3}$$

For the purposes of comparison, I have also rewritten the Ames penetration formula by replacing the speed of sound, c, by its equivalent, the square root of the ratio, Young's modulus divided by the density, namely

$$\frac{p}{d} = 2.28\left(\frac{\rho_P}{\rho_T}\right)^{1/3}\left(\frac{\rho_P \; V^2}{E_T}\right)^{1/3}$$

In discussing the results of this derivation, allow me to play a dual role and be both author and critic.

The critic would remark: "This is all very interesting, but how do you propose to determine s ?"

I would reply: "I would perform an experiment to determine the deformation stress of each material that will be used for a target; this is essentially what has to be done, of course, to obtain values of the speed of sound or Young's modulus for any material, since ordinarily these are empirical quantities. In determining s, however, my experiment would consist of firing a sphere into a massive target at a velocity high enough to be in the fluid impact region. Both sphere and target would be the same material, preferably. I would measure the penetration and compute s from the formula

$$s = \frac{3}{8\pi} \frac{m_p V^2}{p^3}$$

If I am not able to perform this experiment, I would estimate the penetration from the rewritten Ames impact formula using the modulus of elasticity of the target material."

The critic would ask: "But will not the original penetration formula do all that you claim for your new formula? If I can perform an experiment, I can as readily adjust the value of the constant 2.28 as compute your so-called "deformation stress"; and, if I cannot, we both have to use the same formula for a first estimate". And he would add: "You have told me nothing that I did not know already. Your theory is a swindle."

To which I would answer: "My friend, you have expected more than I promised. I have proposed a qualitative model of the impact process, not a precise theory. It is intended to stimulate your physical intuition, not your undoubted capacity for exact mathematical derivation. At the worst, you are no worse off with the new formula than you are with the old. And I have saved you from the disillusionment of expecting great changes as the velocity exceeds the sonic value. At best, some person far cleverer than I am may seize upon my model and provide us with a relation between the deformation stress and the properties of the target material determined from the familiar static tests. Such a relation taken together with the model proposed here will be a real step forward."

Dr. von Karman tells a story about a conversation which he had with a friend from his native Hungary. The friend, a man of letters, had been invited to give a series of lectures in this country. Karman told his friend that he would have a hard time putting his ideas across. "Your accent is very bad. No one will understand you." His friend replied, "Well my accent may be bad, but my emphasis is good!"

Lunar Craters

Let us turn our attention now to the subject of lunar craters. This branch of selenography is a fascinating field of study by itself and has been a prime object of investigation by astronomers for many years. The craters have been mapped in great detail, and many facts are known about them. Our research on impact has just begun to border on the conditions which prevail in nature. It can do no more at the moment than make tentative suggestions about the effects of meteoroid impact and draw a few inferences on their connection with lunar topography.

The results presented so far are for impact in ductile metals. Now, it is very unlikely that the lunar surface is a ductile metal; rather, the moon's crust is believed to be part hard igneous rock, like granite, and part porous stone, like lava. We need to experiment with impact in rock in order to study lunar craters.

Figure 8 shows the crater formed in granite by a 1/4-inch nylon sphere striking at 17,500 ft/sec. For purposes of comparison the figure also shows the crater in 24-ST dural made by a similar sphere at 18,900 ft/sec. Face views of the craters are shown at left. The targets were sectioned and photos of the sections are shown at right to illustrate the craters' profiles. A further comparison is facilitated by tracing a profile of the dural crater on the photograph of the granite section. It should be noted that the granite crater is a little shallower in the center than the photograph shows because some of the granite in the crater's center crumbled away when the target was sectioned.

The difference in size is the most evident contrast between the craters in granite and dural. The granite crater is almost twice as deep and three times wider at the rim than the dural crater, even though the velocity of impact was a little lower. The granite at the outer edges of the crater shows signs of being fractured and pulverized and no raised lip is formed as it is in dural. These last two differences are probably surface effects, since granite is a non-ductile material and would fracture rather than deform as powerful pressure and shear waves sweep outward from the point of impact. On the other hand, the center region of the granite crater is similar to the dural crater, and it is reasonable to suppose that the same physical processes resisted the formation of the craters in both materials.

Since we have only one high-speed shot in granite, a rational approach is to assume that the model of fluid impact correctly describes the formation of the crater and to compute the deformation stress in granite.

The value computed is 16,000 psi, which, incidentally, agrees closely with the value listed in Mark's Handbook for the compressive strength of granite. The deformation stress in the 24-ST dural was also computed from the crater shown in this figure; the value obtained is 80,000 psi, which also seems reasonable and gives additional support to the theory.

If our experiments in the laboratory have any relation to conditions on the moon, they must first be able to account for the craters made by meteorites striking our earth. Now, Figure 9 shows an excellent example of a large meteorite crater, the Barringer crater located in Arizona, which has been studied in considerable detail and should provide a test for the application of our research. The principal question asked about the Barringer crater seems to be: "What was the weight of the meteorite?" In fact, everyone interested in meteorites seems to have had a go at this one, and the weight estimates range from 10,000 tons by Rinehart and Wylie to 5,000,000 tons by Opik and Rostoker, with Whipple's estimate of the most reasonable value being between 80,000 and 400,000 tons. There is also a question as to whether the main mass of the meteorite still lies buried deeply beneath the bottom of the crater.

The Barringer crater is a bowl shaped depression about 4,100 feet across the top and 600 feet deep, with a raised rim rising 200 feet above the surrounding plain so that the bottom is 400 feet below the level of the plain. The crater is estimated to have been formed about 50,000 years ago, and its age makes it difficult to estimate its original depth. However, it could hardly be deeper than 800 feet, if the crater in granite is indicative, and the original depth probably lies between 400 and 800 feet. The meteorite is known to have been made of iron and nickel, since the surface of the earth in and surrounding the crater is richly embedded with these metals and their oxides. We can also make a fairly good guess as to its velocity. It was undoubtedly of planetary origin and probably came from the belt of asteroids. Whipple has estimated that the average velocity of such meteorites is 50,000 ft/sec. The asymmetry of meteoric material around the crater indicates that the meteorite struck at an angle to the vertical, and, for lack of better information, we have assumed the angle of fall to be 45 degrees. Substituting this information into the penetration formula derived from the model of fluid impact with the velocity replaced by its vertical component as required by oblique impact, we obtain a weight of the meteorite of 5,000 tons, if the crater depth is 400 feet and 40,000 tons, if the crater depth is 800 feet.

Now, it so happens that the Smithsonian Astrophysical Observatory sent an expedition headed by John Rinehart to the Barringer crater during the summer of 1956 to survey the soil around the crater and to determine the amount of meteoric material deposited in the neighborhood of the crater. Rinehart sampled the surface soil in a systematic manner and extracted the meteoric material from his samples. He then computed the total weight of iron and nickel that must have come from the meteorite to be 12,000 tons. Our experience with fluid impact would indicate that this is a fair estimate of the total weight of the meteorite itself, because the meteorite would fracture immediately on impact and the fragments would flow out on the surface as the crater is formed with part remaining in the crater and part being thrown out over the rim with a shower of boulders and rock flour.

Rinehart's value of 12,000 tons is bracketed by our values of 5,000 and 40,000 tons. The agreement is encouraging and probably signifies that the predictions of the theory are the right order of magnitude. As far as any residual mass of the meteorite buried deep beneath the crater is concerned, the authors would advise caution in investing in any mining company which expects to recover a fortune in iron and nickel from this particular corner of Arizona.

It is a long step from the Barringer crater to the craters of the moon. First, let us view one of the most prominent of the lunar craters. Figure 10 shows the full moon photographed by the 100-inch reflector at the Mount Wilson Observatory, a truly beautiful picture. The crater I refer to is Tycho, clearly visible even at the small scale here. This is the crater itself; note that it is surrounded by bright streaks, called "rays" which radiate out from the crater like the spokes of a wheel from the hub. Just what the rays are is still a mystery.

If you will recall the photograph of the crater in granite, I believe you will recognize a certain similarity. Both have the appearance of a shallow, rough cavity in rock. The gross similarity between the two suggests they both may be the result of high-speed impact. But this is as far as we can reach with our limited knowledge of impact. There are many points of difference. Tycho is much shallower than our granite crater, it has a raised rim, and a mountain peak appears at its center. These differences are matters of future study.

You will recall, also, that our granite crater was not surrounded by rays. But this difference can be explained. If one fired a sphere into a level granite surface on the moon, he would expect to find a large area, centered at the crater, covered by a powder of rock flour interspersed with fragments of various sizes.

To illustrate this point, we have taken high-speed motion pictures of the impact in granite, and the first few frames of the movies are shown in the next figures. The projectile was a copper sphere fired at 6,000 ft/sec. A Fastex camera was used running at 5,000 frames/second, but this rate is so slow compared to the speed of the impact that it gives only a gross picture of the process. The granite block was placed so that its surface was vertical. Figure 11 shows the surface of the granite before impact. Figure 12 shows the first frame after impact. Note the shower of fine particles being thrown out from the crater. This shower is principally fine rock flour, which one finds deposited on all the surroundings of the target after the shot. Figure 13 shows the second frame after the impact. Note how rapid the shower flies out. Its maximum velocity was estimated to be 1/3rd that of the projectile. Figures 14, 15 and 16 show later frames; the time starting from the frame of the first figure is noted at the top. After the shower dissipates, a column of dust and larger fragments seems to rise centrally out of the crater.

If the impact occurred on the moon, the rock dust would fall back and eventually lie undisturbed on the surface where it might be seen as a bright, crystalline powder against a dark surface. The velocity of the shower can only be guessed at but, if it is an appreciable fraction of the meteoroid's velocity, it could cover great reaches of the moon's surface, since the lunar escape velocity is only 8,000 ft/sec and there is no atmosphere to retard the shower in its flight. Also, the lunar landscape is very rough, and, if the terrain near the impact contained jagged peaks, rills, and other sharp discontinuities the pattern of the rock showered out from the crater would probably be quite ragged. The meteoroid itself probably has an irregular shape. The impact is in the fluid region, of course, and grooves and ridges in the impacting surfaces would form jets of powdered rock that would squirt out in various directions forming an irregular pattern centered on the area of the crater (but not necessarily the center of the crater). The jets could form the rays. The powdered rock from the jets would fall to the surface and appear as a bright ribbon stretching across the lunar landscape.

Conclusions

These inferences on the nature of meteoric craters are a long reach at best from the meager results of our research on high-speed impact. The authors consider that the model of fluid impact, which they propose, to be no more than a bare beginning towards a real understanding of the physics of impact at meteoric speeds. Further progress will depend on the development of an adequate theory and on experiments made at the full orbital speeds of meteroids.

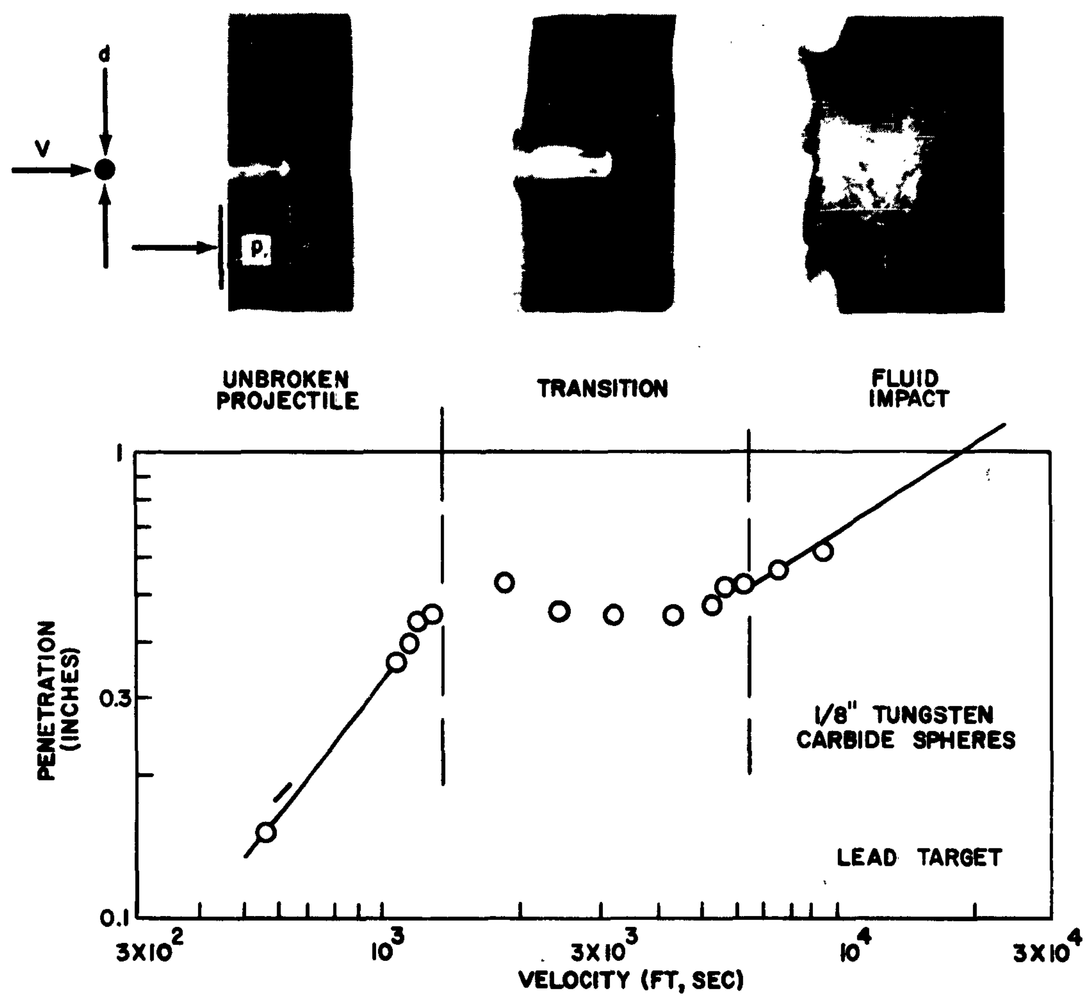


Fig. 1 Basic Types Of Impact

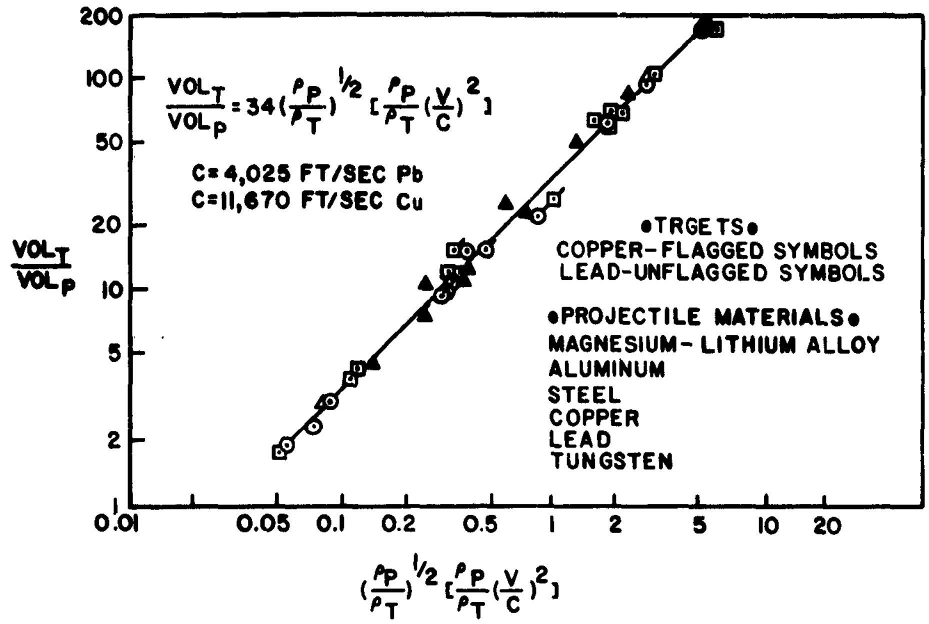


Fig. 2 Correlation Of Volume Data

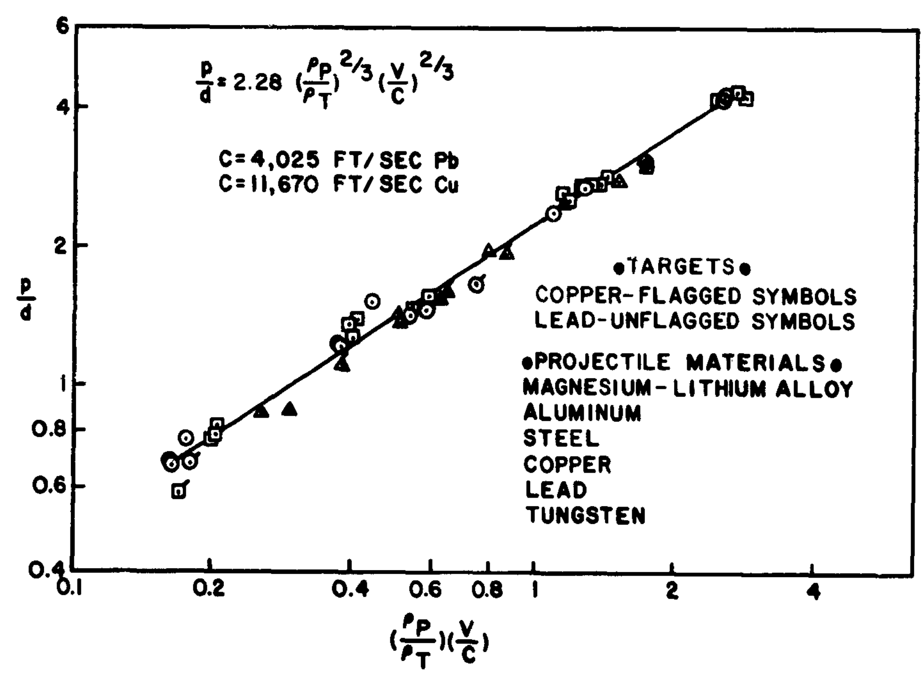


Fig. 3 Correlation Of Penetration Data

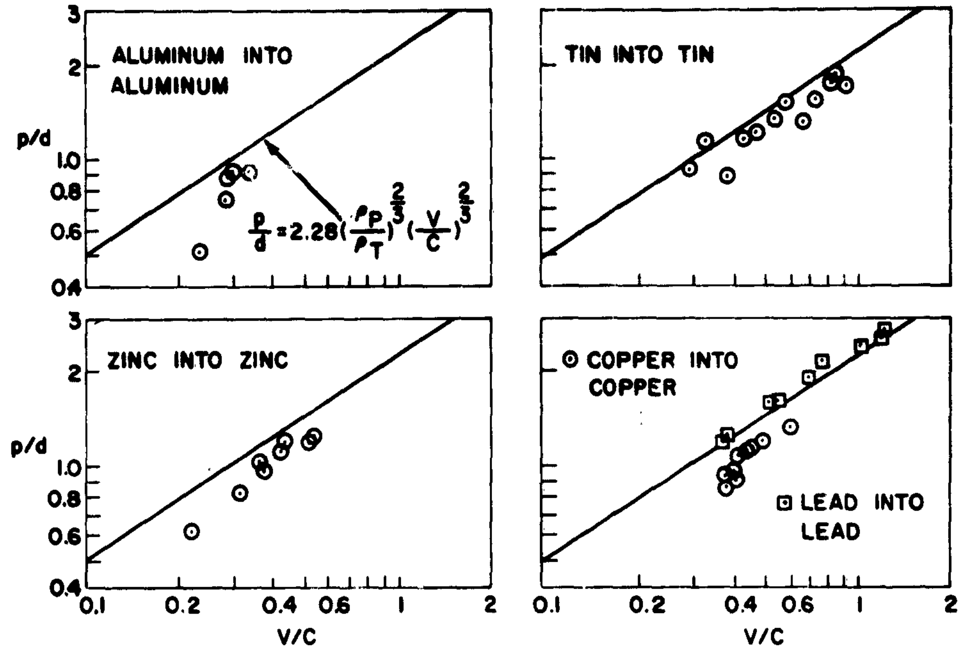


Fig. 4 Penetration VS Velocity: Spheres - U. Of Utah

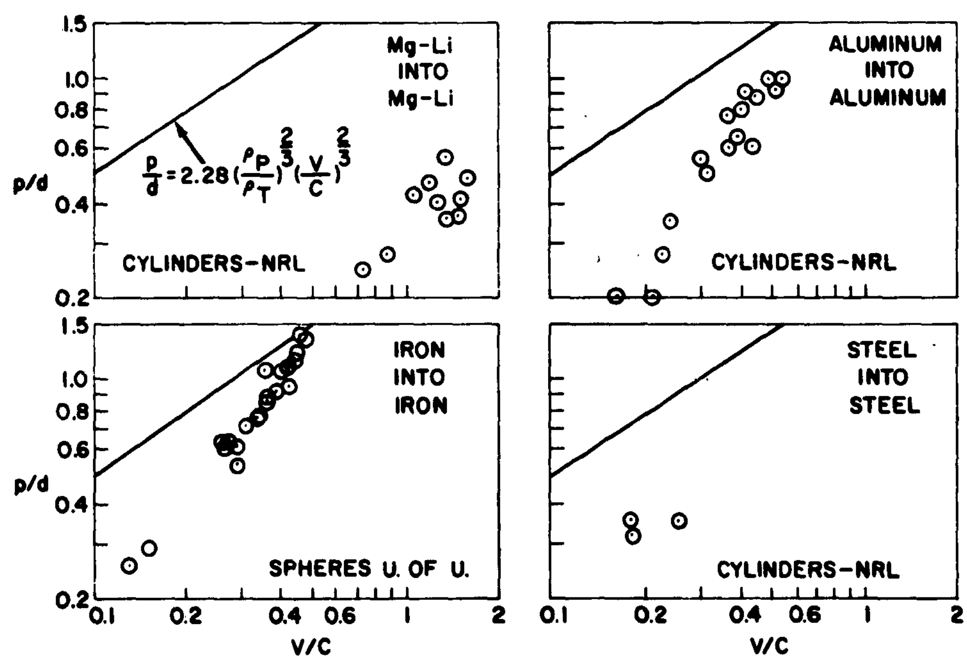


Fig. 5 Penetration VS Velocity

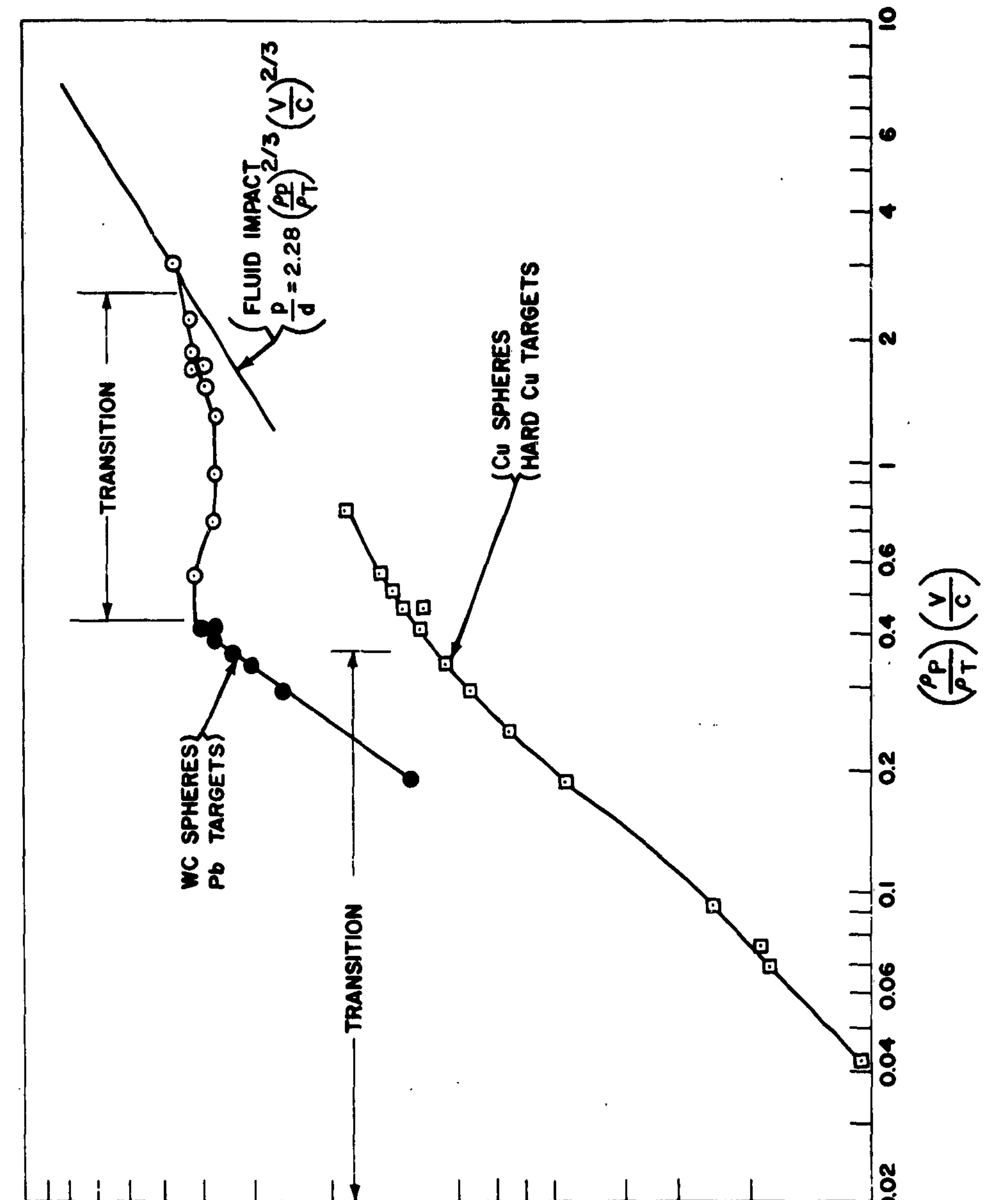


Fig. 6 Impact Transition

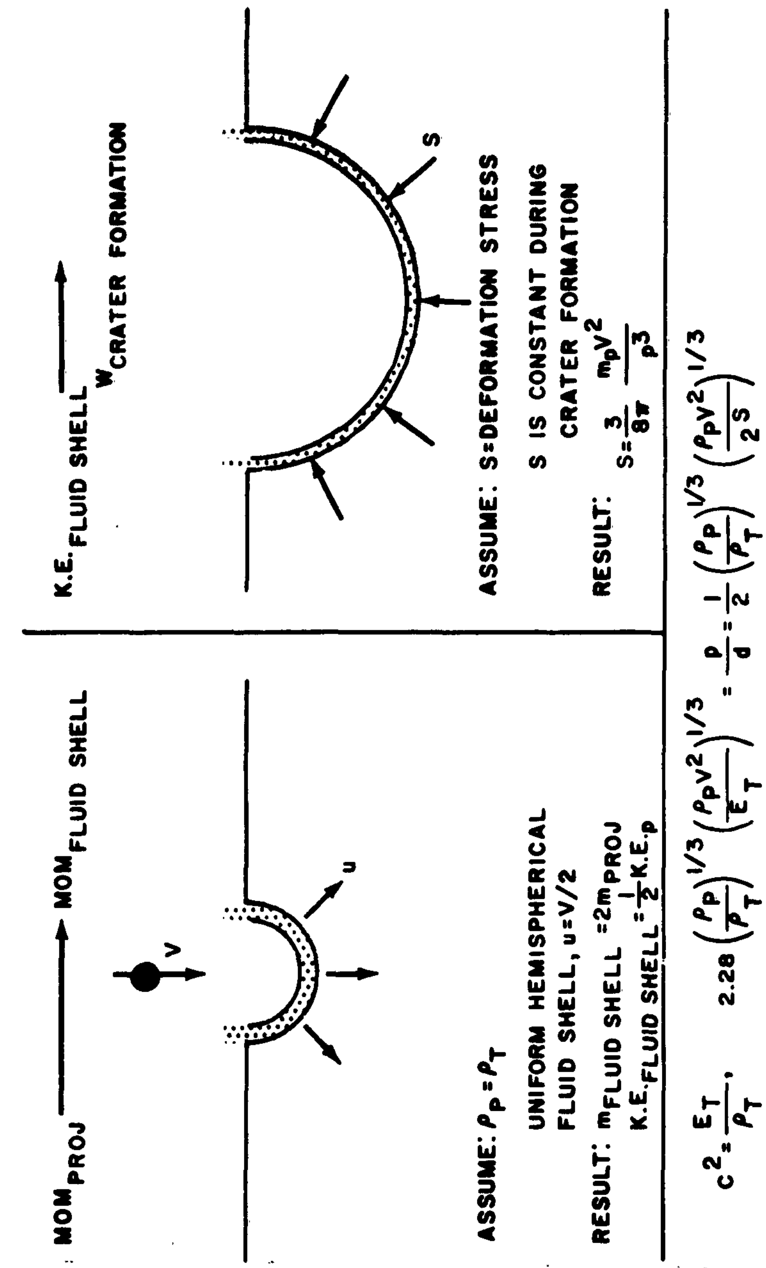


Fig. 7 Ballistic Pendulum Analogy Of Fluid Impact Crater Formation

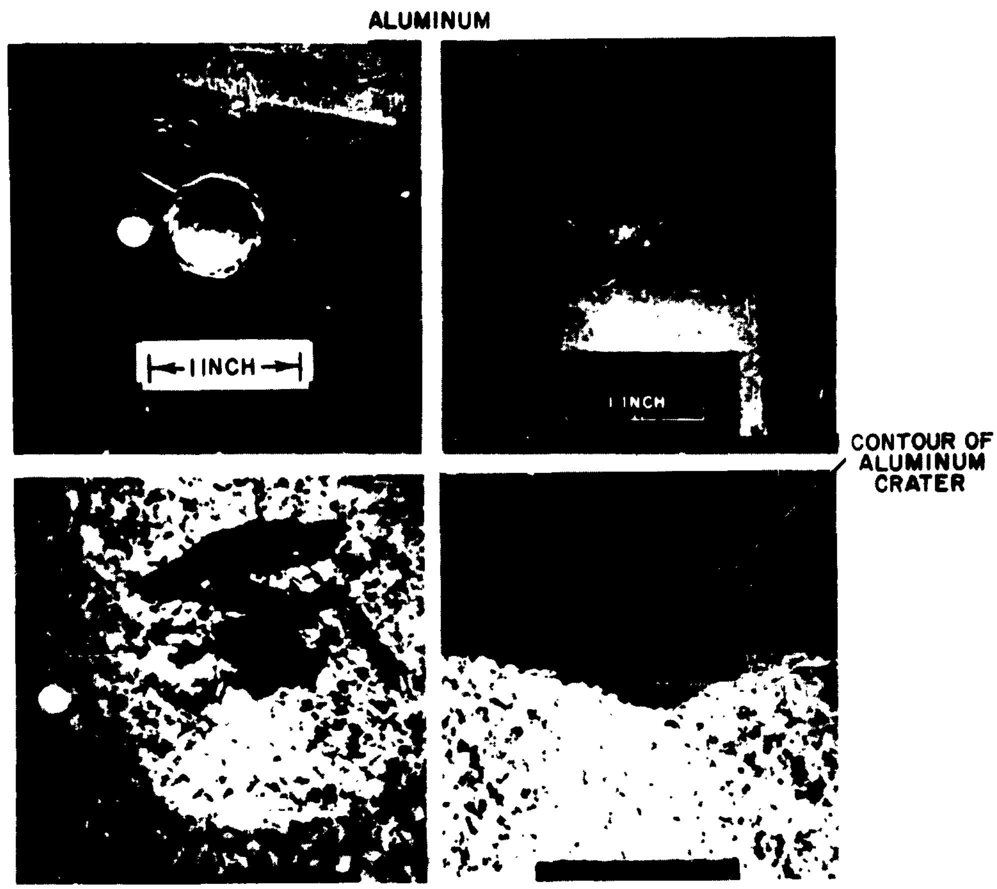


Fig. 8 Comparison Of Fluid Impact In Aluminum And Granite

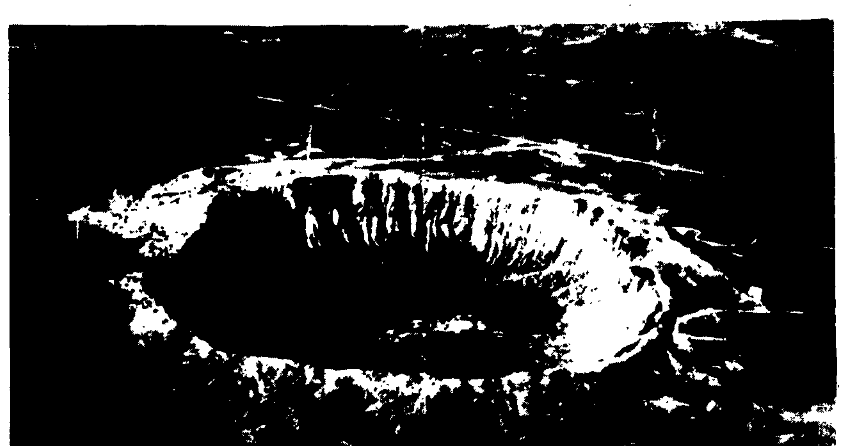


Fig. 9 Barringer Crater

Fig. 10 The Moon

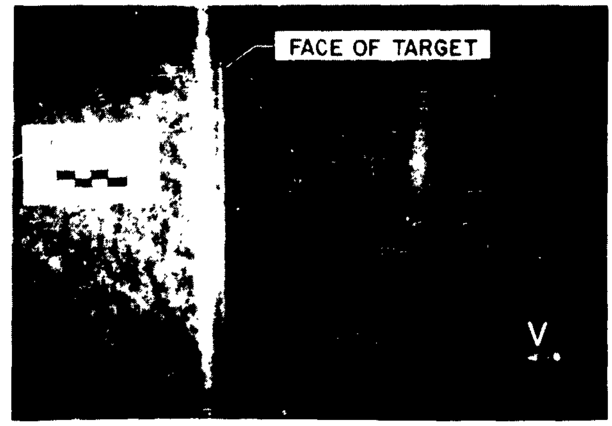


Fig. 11 Impact In Granite - Time: Zero Sec.

Fig. 12 Impact In Granite - Time: 0.0002 Sec.

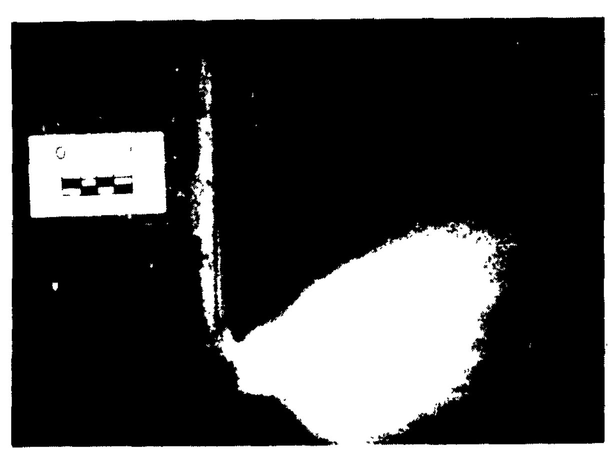


Fig. 13 Impact In Granite - Time: 0.0004 Sec.

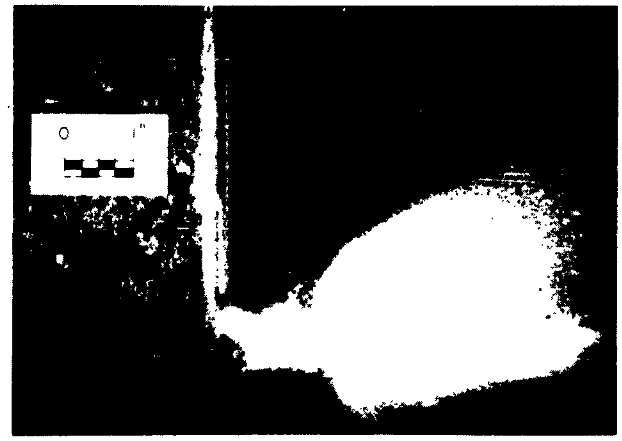


Fig. 14 Impact In Granite - Time: 0.0006 Sec.

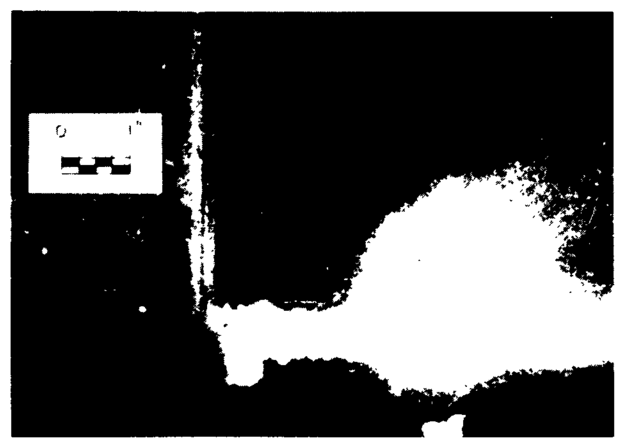


Fig. 15 Impact In Granite - Time: 0.0012 Sec.

Fig. 16 Impact In Granite - Time: 0.0018 Sec.

MILLISECOND MEASUREMENT OF FORCES AND MOMENTS IN HYPERSONIC FLOW

by

Dr. Arnold E. Seigel
Chief, Gas Dynamics Division
U. S. Naval Ordnance Laboratory

After listening to the eloquence of the preceding two speakers, I feel that perhaps I should have stayed in bed today, and maybe you'll have that feeling too, in a few minutes. When I was asked to submit the title of the talk four months in advance of this symposium, I had a choice to make. I could either plan to present work that we had already completed, or present work which we hoped to complete by the time the decennial came around. I took the latter course of action assuming that it would be of more interest and novelty to you - of course I don't want the novelty to be my standing here with nothing to say. So as time went by, my hope became a prayer, and I find myself in a position today where my prayer has not been completely answered; we have not completed the work which I want to discuss with you, but we do have some initial results which I would like to present.

In this space and missile age, as Dr. von Braun has just told you, there is a continuing need for more and more aerodynamic information at higher Mach numbers, higher velocities, and so on. I can visualize a politician in the not-too-distant future reading the newspapers, seeing the strides Russia is making, and running to NOL saying we need information at velocities higher than the velocity of light. We would, of course, furnish him with that information.

The particular problem that we attacked was to determine whether it was feasible to measure forces and moments at high Mach numbers, say above Mach number 10, and at densities up to one-tenth room density. This is a region of Mach number and density which the usual ground laboratory facilities (such as ballistic ranges and wind tunnels) cannot reach at the present time. The ballistic range potentially offers the means to simulate air flight under these conditions of Mach number and density. However, at the present time it is very difficult to launch a model at greater than about 10,000 feet per second, especially if the model is complex, fragile, or has wings. Also in the ballistic range, one must launch a model that is stable. The wind tunnel also has its limitations. At the present time, wind tunnels cannot go beyond a Mach number of about 10, and at this Mach number the density is very very low. The shocktube wind tunnel (sometimes termed the "hypersonic shock tunnel") however, does offer a possible solution to the particular problem that we are interested in, that is, measuring forces and moments at these conditions of high Mach number and density.

The shocktube wind tunnel contains a driver gas initially at high pressure which is separated by a diaphragm from air in a tube at a lower pressure. When the diaphragm ruptures, the driver gas pushes the air down the length of the tube (or barrel). This pushed air is heated and compressed and finally is discharged out of the tube and expanded into a test section. As it expands, the air Mach number increases from about 2 to the Mach number at which we want to test, say 10 or 12; but at the same time the air density decreases enormously in the order of 10,000 times, and as Dr. Slawsky said this morning, you cannot test with 17 molecules or even 34 molecules. Thus, in order to measure forces and moments, we needed relatively dense air in the test section. This was our aim.

In order to obtain this large density in the test section, we had to start with initially very large density in the tube itself. Having a density which was large in the tube meant that we needed a high-pressure driver in order to drive this dense air. Consequently, we find that our shocktubes began to resemble guns. They were constructed to be capable of withstanding 60,000 psi working pressure. In fact, at NOL we now use gas guns and shocktubes interchangeably. As the head of a shocktube group I find that this situation is very fine. We may now, anytime we desire a shocktube, go to the gun people and borrow a gun. I have no sympathy for the reverse situation.

The disadvantage of a shocktube wind tunnel is the short duration of flow time available in the test section. Because this flow time may be only a few milliseconds, we selected a method employing a free model in the test section. Now in the short, say, 2 milliseconds of flow time, the effect of gravity on a free model would be small relative to the effect of the forces flowing over this model. For example, in 2 milliseconds, a model would drop less than two-thousandths of an inch. Thus the method which we proposed utilized very light models which were suspended by fine threads of wire in the test section of the shocktube wind tunnel. When the expanding air first reached the model, the suspension wires were swept away leaving a free model whose subsequent motion was determined only by the air flowing past it. Of course, if the wires didn't break, we would have the initial phase of a boxer's punching bag impact; since we're not in the gymnasium business, this would do us no good. Fortunately, in our case, the wires did break.

To observe the motion of the model in the shocktube wind tunnel we used a high-speed continuous-writing framing camera. This camera took 80 separate photographs at a rate as high as 1-1/2 million frames per second. Thus, the resultant situation was that effectively the shocktube wind tunnel became a ballistic range with 80 closely-spaced observation stations. The procedure then was to take these photographs and analyze them to find the motion of the model. The framing rate between the frames told us the time between frames and, therefore, we had motion-time history of the model. From the motion-time history of the model, we could deduce the moments and forces acting on it. We even hoped to get the damping forces acting on the model.

Now, I would like to talk about the apparatus we used in this experiment. Figure 1 is a drawing of the 4-in. Hypersonic Shock Tunnel. The chamber is a modified Navy 8-in. gun which is now 14 feet long and has an inside diameter of 10 inches. It is connected to a barrel* which is 60 feet long and 4 inches in inside diameter, thus it derives its name, 4-in. Hypersonic Shock Tunnel. Both the barrel and the chamber are built to withstand 60,000 psi working pressure. The barrel extends into a receiver tank where the test section is situated. We have two windows, one rectangular and one circular. At the circular window an operator is shown looking through the high-speed camera. In the lower left of this Figure is a section of the test section, sort of a test-section section, where we see the two windows and a freely suspended model. In this case, we have a futuristic type of winged model. Figure 2 is a photograph of the high-speed framing camera.

A schematic of this shocktube wind tunnel is shown in Figure 3. Here the driver section is shown on the left rather than on the right as it was in Figure 1. The driver gas was helium which was heated by the reaction of hydrogen and oxygen, resulting in a steam-heated helium mixture. This mixture had a sound speed of 7,000 feet per second. It was contained behind the diaphragm, shown hemispherical, which was designed to burst at about 12,000 psi. It was a non-shatterable type, designed to fold back so that none of the diaphragm fragments would go down the barrel. The peak chamber pressure after the reaction was about 25,000 psi. In the barrel section, the air was contained and initially loaded at a relatively high density - 30 times room density, 450 psi. This was in keeping with what I have said, that in order to have high density in the test section we must start with high density in the barrel. At the end of the barrel a slightly divergent section is shown. This section diverged from 4 inches to about 7 inches in diameter at the open

*The tube of the shocktube wind tunnel will henceforth be referred to as the "barrel".

end where we have a diaphragm. This diaphragm provided the means for separating air from the vacuum in the test section. This divergent section was designed so that any disturbances from the opening diaphragm would diverge and thus not enter into the vicinity of the models. In the test section we see three models suspended, a sphere and two cones, all 8 feet from the end of the barrel. As you know, we expand the air from the barrel to the test section not through a nozzle but as a free expansion. (We hope to put in a nozzle at some future time but at the moment we are doing it in this manner.) Theoretical calculations had indicated that there was a region near the axis of the test section where the flow was uniform and parallel. In order to confirm this visually we hung in our test section some test spheres as shown in Figure 4, perpendicular to the flow. The resultant schlieren photograph is shown in Figure 5. If we look at the shock waves carefully we note that there is an angularity to the flow. The sphere, which is second from the bottom, is about in the center line of the flow. The theoretical calculations indicated that for a movement of about 4 inches axially there would only be a change in the Mach number 11 range of from 11 to 11.2 and there would be hardly any change in the Mach number at a perpendicular distance of 4 inches from the axis. The idea we had in mind was to suspend small models near the axis and test in this fashion. These models would be in a uniform parallel flow in a portion where the Mach number would vary only slightly.

The models we decided to use were cones. We decided to use cones because there had been theoretical treatment of the air flow over cones published by Taylor and Maccoll, Kopal and others which we could use. Also the drag and moment coefficients of cones did not vary too much with Mach number. Therefore, we could compare the results of our experiment at high Mach numbers with the results of lower Mach number experiments in conventional wind tunnels and ballistic ranges. The cone vertex angle which we decided to use was 60°. This is a relatively large angle and was chosen because we wanted the center of pressure to be back as far as we could make it. This would mean that the cones would be more stable and would oscillate relatively more in the short flow time that we had in which to perform the experiment.

You might note that from a side view a 60° cone looks like an equilateral triangle. We couldn't tell the nose from the top from the bottom. I think this gave rise to the remark describing our work that we didn't know which end was up. But actually we did know which end was up, because in the pictures we saw the shock waves around our cones and thus we knew which was the front and which was the back.

We didn't want too large a cone vertex angle because if it were too large the cone at an angle of attack would have a detached shock and this was undesirable. Another reason we didn't want too large a vertex angle was that the cone would be swept downstream too great a distance because its drag coefficient increases as the vertex angle increases. We therefore suspended one-inch diameter cones with very small moments of inertia in the test section, and we hoped they would be in a uniform parallel flow. The results of the schlieren photograph, Figure 5, had indicated that actually a good approximation to the actual flow was a source flow with the source at the barrel mouth. Figure 6 is a photograph taken while our models were being suspended. Here we are looking from the test section toward the barrel. The large visible circle is the divergent part of the barrel, and the shiny portion in the middle of that circle is the diaphragm at the end of the barrel. There is a cone almost at the center line suspended at an angle of 7° from the horizontal, and there is another cone away from the center line suspended at an angle of 20° from the horizontal. The wires that are holding these cones look rather thick here, and it would appear that the previously mentioned punching bag type of motion would occur, but actually it was the reflection of the light on these wires that gave this thick appearance. The wires were just strong enough to support the weight of the cones. The sphere visible here was put in to determine the dynamic pressure in a manner which I will discuss in a few minutes. The sphere, made of nylon, was one-half inch in diameter; the cones of ethyl cellulose were one inch in diameter.

The procedure was to darken the shocktube wind tunnel room and run the camera at such a speed that 80 photographs would be taken for our 2-1/2 milliseconds of flow duration. Then we pressed the buttom which ignites the mixture, and we always accompany this with a slight prayer, for the big guns a big prayer, and for the little guns a little prayer. This is because we had much more difficulty with detonation with the larger guns. With the small shocktube wind tunnels we seem to be able to operate without detonation problems. We had had one detonation in a small shocktube wind tunnel which almost knocked down a building, and we realized that if we had a detonation with this large shocktube wind tunnel the entire building would certainly be blown up. There was also a very disturbing factor in that the location was poorly selected for this particular shocktube wind tunnel which was directly under my office. So I was praying much more than the other people were praying.

After the driver gas mixture was ignited, it pushed the air down the barrel. The shock wave, upon passing the pressure gages in the barrel, triggered a delay unit which energized the light source at the correct time; this illuminated the test section during the flow period and exposed the photographic film in the high-speed camera. You note, the important result we get out of such an experiment is the photographs, the 80 photographs, and I continually reminded the photographer, "Do you have film in your camera?" He resented that, but I continued to plague him with the question.

Figure 7 shows one of the frames we took in an experiment where we suspended the cones. The two cones and their shock waves are quite visible. Actually the sphere and the cones are not as close to one another as it looks, because in our manner of suspension they were displaced perpendicular to the plane of the photograph as well as vertically. The fiducial system is seen in the background. It is a horizontal-vertical lined grid system.

A composite of six of the photographic frames is shown in Figure 8. Since this shot was taken we have taken another shot in which the photography is improved. You will note in the six frames the oscillatory motions of the cones. We found that we had more than 1-1/2 complete oscillations. The sphere seems to have been left out of these photographs although it was in most of the original strip of negatives. I think I told the photographer to "get on the ball" and he just took off the ball entirely. The thing to note here is that the two cones started their motion with the lower one ahead of the other, and during the air flow the upper cone begins to catch up with the lower one. This is due to the fact that the two cones had different drag coefficients as a result of the fact that the upper cone was suspended initially at a higher angle of attack than the lower cone. We made a little movie out of these photographs. In the first part of the movie we repeated each frame three times, so instead of 80 frames we have 240 frames and one may see visually the motion of the cones. The second part of the movie was made by repeating each frame 10 times; as a result we get a much slower and jerkier motion, and it becomes sort of a jerky movie.

We haven't completely analyzed the results of these experiments, but we have extracted some data and I would like to discuss these with you. The test section conditions were approximately the following: the dynamic pressure was 40 psi, the static pressure was 1/2 psi, the Mach number was 10.5, and the velocity was about 6500 feet per second. To achieve the same conditions in the conventional wind tunnel one would have to have the air in the reservoir at a pressure of about 60,000 psi and a temperature of 4500°F. Figure 9 shows a travel time plot for the sphere. The ordinate is distance units on the measuring machine and can be converted to inches. We have plotted these distance units against time in frame numbers with 28 microseconds being the time per frame. So actually this is a X-T diagram for the sphere. I mentioned that in the early part of the movie there was no sphere, and therefore we could not record any of the sphere data in the initial parts of the run; however, there were sphere data in the end and we were able to fit an analytic curve through these data. The second experiment enabled us to get the entire sphere picture as shown in Figure 10. These data look very nice to us and even look nicer if you superimpose the results of three peoples' measurements on one plot. From the sphere data we were able to calculate directly the acceleration of the sphere. The acceleration of the sphere is proportional to the q (i.e., $1/2\,\rho V^2$) in the test section times the C_D of the sphere. In our Mach number range the C_D of the sphere was well known from ballistic range data. Therefore we could solve for q, the dynamic pressure, in the test section.

The method of obtaining the Mach number in the test section is illustrated in Figure 11. Here we have plotted pressure of the expanding air versus Mach number of the expanding air as it expands from the barrel to the test section. P_m is the muzzle pressure and M_m is the muzzle Mach number just before it expanded. We measured muzzle pressure with our pressure instrumentation and the shock Mach number with some ionization gages and we could check the correspondence of these two numbers by actually using the conservation laws across a shock. From the shock Mach number we could then calculate the air Mach number M_m. From the point P_m, M_m we drew an isentropic line which we assumed to be the path of the expanding gas. The data for the isentropic expansion were taken from the Bureau of Standards data for air. If we draw constant q lines on the same diagram they would appear like the dashed lines in Figure 11. We have $q = 30$, 40, and 40 psi. The intersection of the isentrope with the proper q obtained from the sphere data gave us the Mach number and the pressure in our test section. This intersection point could be written approximately by the equation shown in Figure 11, and it is to be noted that the Mach number is proportional to $(P_m/q_t)^{1/5}$, the pressure at the muzzle divided by the test section $1/2\,\rho V^2$ to the 1/5th power. This indicated that if we had an uncertainty of say 10% in our P_m/q_t we would have an uncertainty in Mach number of only 2%. Thus, we believe the Mach number in our test section, which came out to be 10.5, is certainty within 2%. This overcomes one of the great difficulties with short flow-time duration types of facilities. In a short flow-time duration type of facility you can measure almost anything you want in the test section - you can measure pressure, heat transfer, and forces and moments, but the problem is the determination of test section conditions. Here we have a way of specifying those conditions quite exactly.

Figure 12 shows a plot of the cone data. Here we have distance versus time for two different cones. One was suspended initially at 7° from the horizontal and the other at 21°. We note that the X-T curves converge indicating that the acceleration is different for each cone. Thus, the method we are using is sensitive enough to detect the difference between a 7° and a 21° initial cone angle of attack. The difference, of course, is due to the difference in the drag coefficient. We took the acceleration of one cone and compared it to that of the other cone from these experimental curves, and obtained a 10% difference. We then calculated what this difference would be from the Newtonian flow approximation, and it came out to be 11%; so we are very happy with that result.

Now, if one takes these data and differentiates them by taking $\Delta x/\Delta t$ we get what is visible in Figure 13. When we differentiate in this manner the $\Delta x/\Delta t$ is the velocity, here given in feet per second, which is the ordinate versus time, as shown in Figure 13. Now these are, mind you, unsmoothed data; that is, derived from raw data. They are simply the interval Δx divided by the time interval and you see the scatter. If we had actually constant conditions in our test section we would expect for each cone a constant acceleration, that is, the slope of the velocity curve should be constant for each cone, but as you see it isn't constant. The slope seems to be disturbed in the middle of the curve.* (I call it the disturbed q situation.) This disturbance really is a result of the fact that everything was not well back in the chamber - you know "back at the ranch things were not well." Evidence of this is the pressure at the muzzle curve versus the time.

Figure 14 is a schematic of what we obtained when we measured pressure at the muzzle of the barrel just before the air expanded, and here we see that the pressure rose to about 8000 psi and then began to drop, which we didn't like, and then rose again. This is typical of a muzzle pressure record when a detonation occurs in the chamber. (This is the "detonation bump.") The pressure begins to drop as a result of the detonation and then rise from the reflection from the back end of the detonation. Thus we weren't getting q exactly constant in our test section because of this detonation, but the beauty of this scheme which I have just outlined to determine drag coefficient is that we do not have to have a constant q. It is only C_D ratios that we are measuring, C_D of cone to C_D of sphere and C_D of cone to C_D of cone, which do not in this scheme depend on having constant q. It doesn't matter what the q is. If the q changes somewhat it shouldn't make any difference between the ratio of the C_D's. This made the scheme very attractive.

*The disturbance is greater than one would attribute to that due to the change in instantaneous drag of the cone due to its change in angle of attack.

Now we calculated the C_D ratio of cone to sphere as shown in Figure 15. On this figure the C_D comparison is listed (we used the cone whose initial angle of attack was 7^o). Also listed is the facility from which we obtained these data, and the Mach number. Our shocktube wind tunnel at the Mach number 10.5, as shown, gave very close results to the ballistic range at a lower Mach number and the inviscid theory with which we compared it. They are all within 5% and we were quite gratified at this.

The angular history of the two cones is plotted in Figure 16. One was suspended initially at an angle of 21^o and the other at an initial angle of -7^o. It is clear that the frequency can be calculated. The number of oscillations in the 2-1/2 millisecond flow-duration time, is about 1-1/2. If one looks at the solid circles (representing the 21^o cone) it is apparent (after I tell you) that these points are not symmetrical about the line of zero degrees. Actually the points are displaced about $2\text{-}1/2^o$. This was a consequence of the fact that the cone had not been put directly at the center line of the shocktube wind tunnel test section, but had been displaced 4 inches from the center line, and we would expact an angular divergence of the flow about $2\text{-}1/2^o$. So the angle that we tested the cone at was not 21^o but about $18\text{-}1/2^o$. The other cone (the -7^o cone) was at the center line and the points were symmetrical about the zero degree line. From such a plot as this it is rather simple to get the frequency and for a small angle, say for 7^o, the frequency is directly proportional to the square root of the q value in the test section times $C_{m\gamma}$, the moment coefficient. Therefore, we took the frequency derived from this plot and we set it equal to the square root of q times $C_{m\gamma}$ times other factors that came in and solved for $C_{m\gamma}$. Figure 17 shows the results that we obtained and they are compared with the ballistic range and the wind tunnel at lower Mach numbers. The $C_{m\gamma}$ seems to compare very well with what we obtained in our shocktube wind tunnel experiment.

I would now like to summarize with Figure 18 what one could obtain in such an experiment as we have done. The quantity q which is $1/2\,\rho V^2$ in the test section we obtained from the sphere motion making use of the known C_D of the sphere. We hope also to confirm this value of q by putting a good pitot pressure measuring device in the test section. The Mach number was then obtained from the q and the muzzle pressure assuming an isentropic expansion from the muzzle. The C_D ratio came from the cone motion compared to the sphere motion and for that ratio we did not have to obtain the q. The $C_{m\gamma}$ for the small angle of cone came directly from the cone oscillation frequency. For the larger angles, of course, we have to go into a more elaborate procedure to determine the $C_{m\gamma}$ which may be a function of angle of attack. I didn't mention damping coefficient but this seems obviously something that one could obtain from such data as we have here. We think we probably need a few more oscillations but the possibility is present to obtain the damping data. Another thing we think we can derive from this sort of experiment is the lift coefficient.

In conclusion, I would like to say that the results of these initial experiments seem very promising to us. We think that by using such a method one can obtain forces and moments in Mach number regions and density regions, which are not accessible to the usual laboratory tools today. We intend to extend the method to a higher Mach number and use higher pressures in our driver chamber. We also hope to improve our photography. And, finally, we are either going to eliminate the detonation problem or change the location of my office.

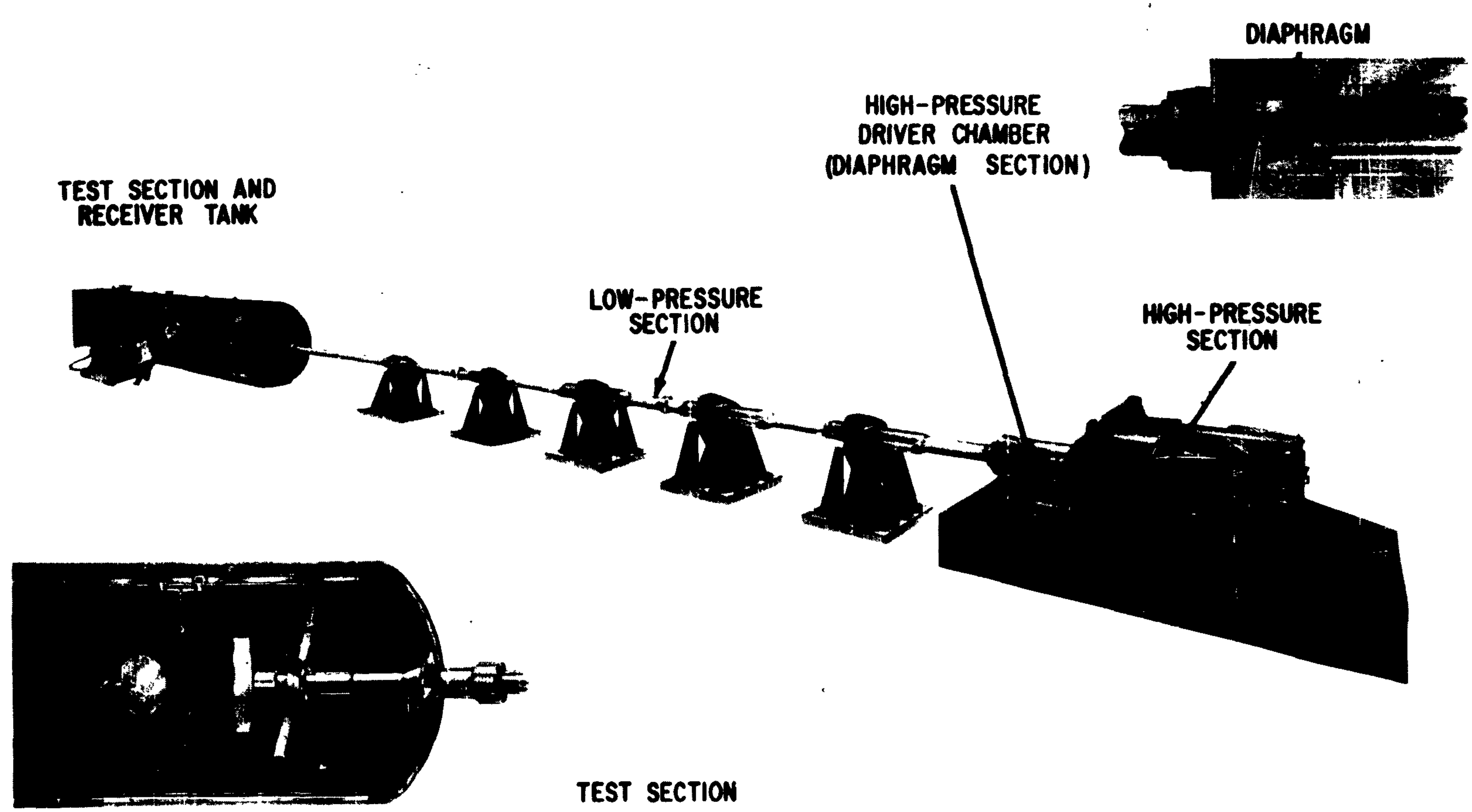


Fig. 1 4-Inch Hypersonic Shock Tunnel No. 3

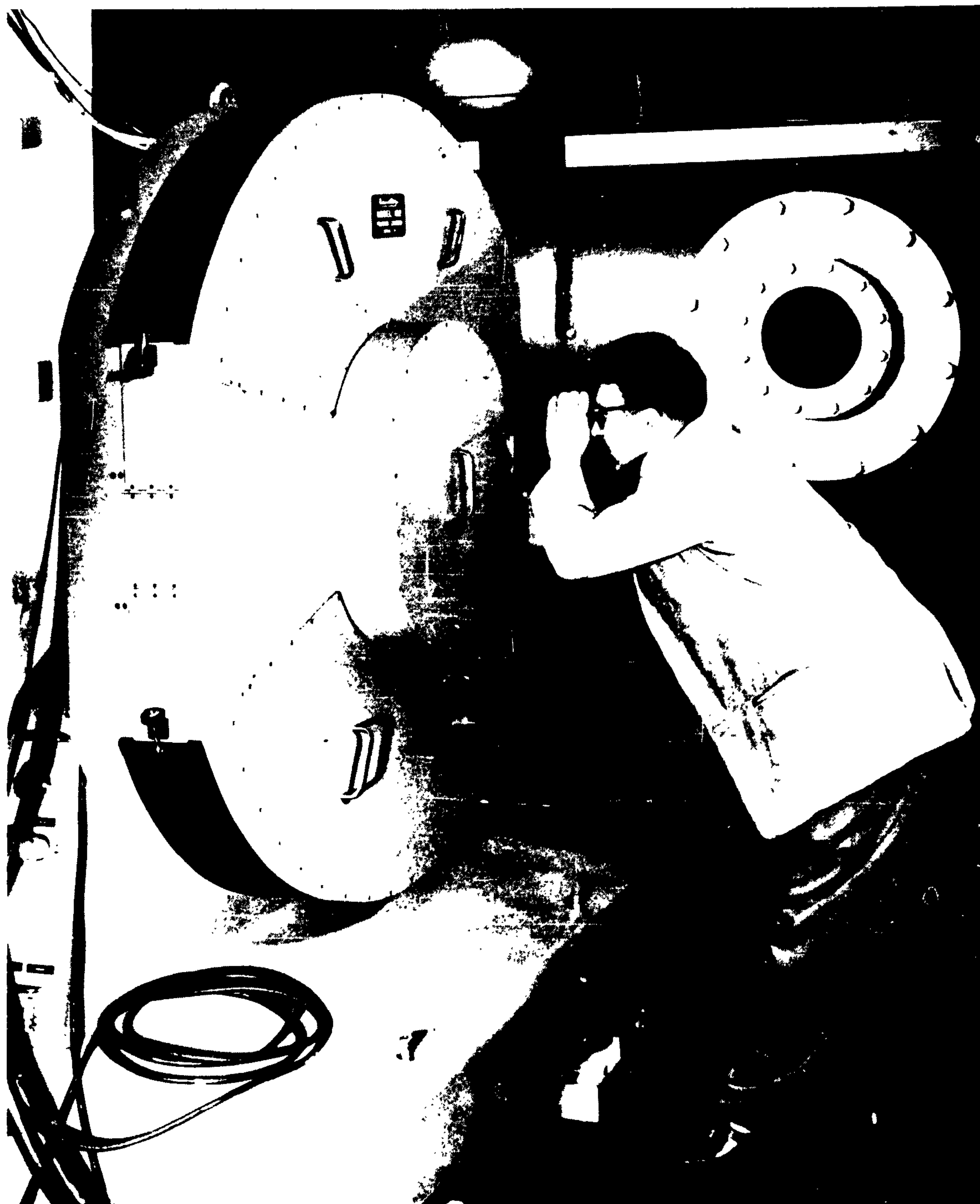


Fig. 2 High-Speed Camera

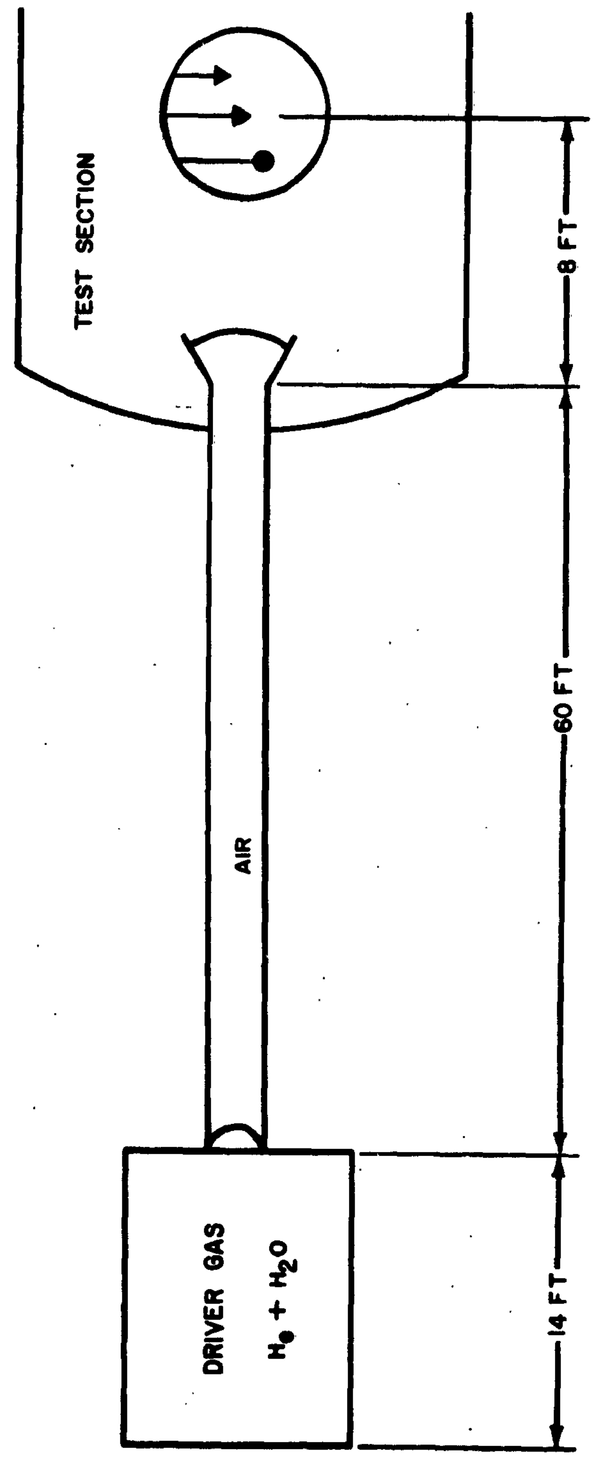


Fig. 3 4-In. Shock Tunnel Test Facility

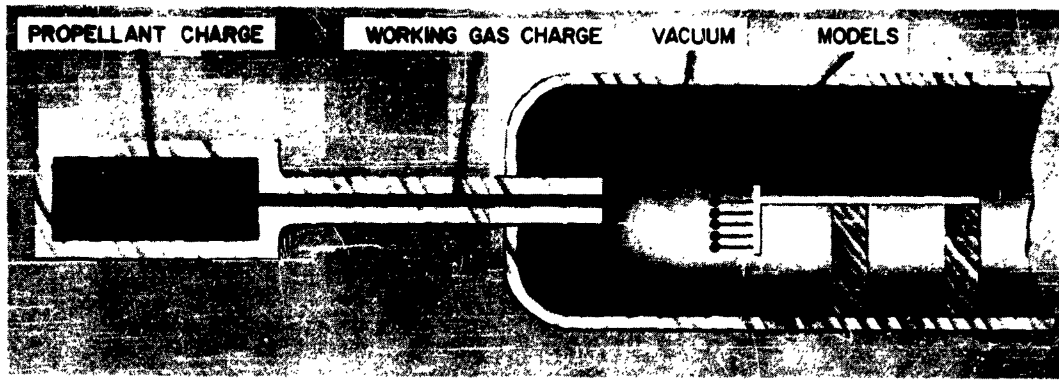


Fig. 4 4-In. Hypersonic Shock Tunnel With Test Spheres

Fig. 5 Schlieren Photograph Of Test Spheres In Shocktube Wind Tunnel

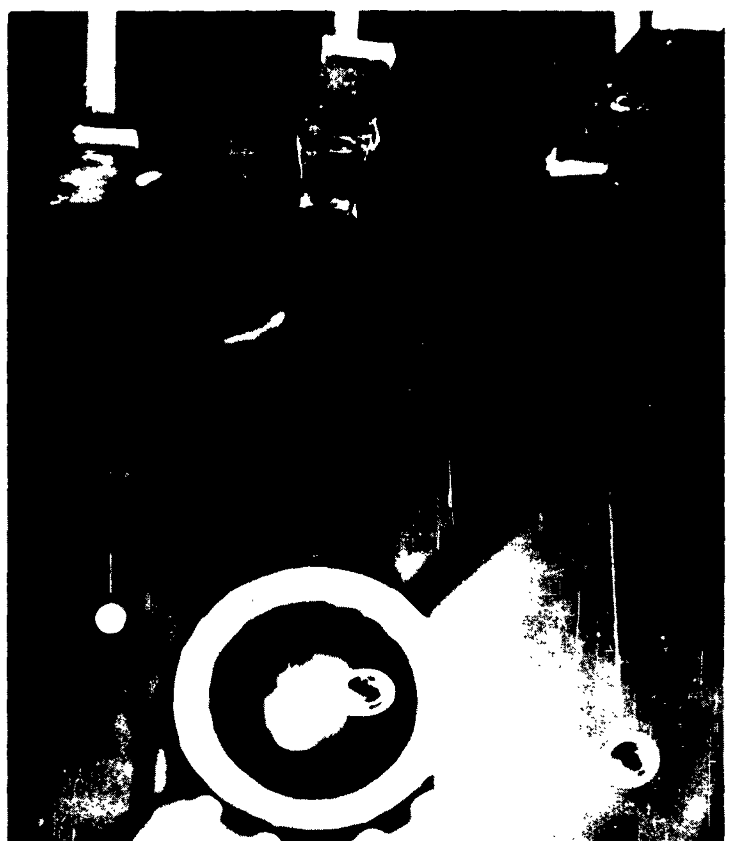


Fig. 6 Models Suspended In Shocktube Wind Tunnel Test Section

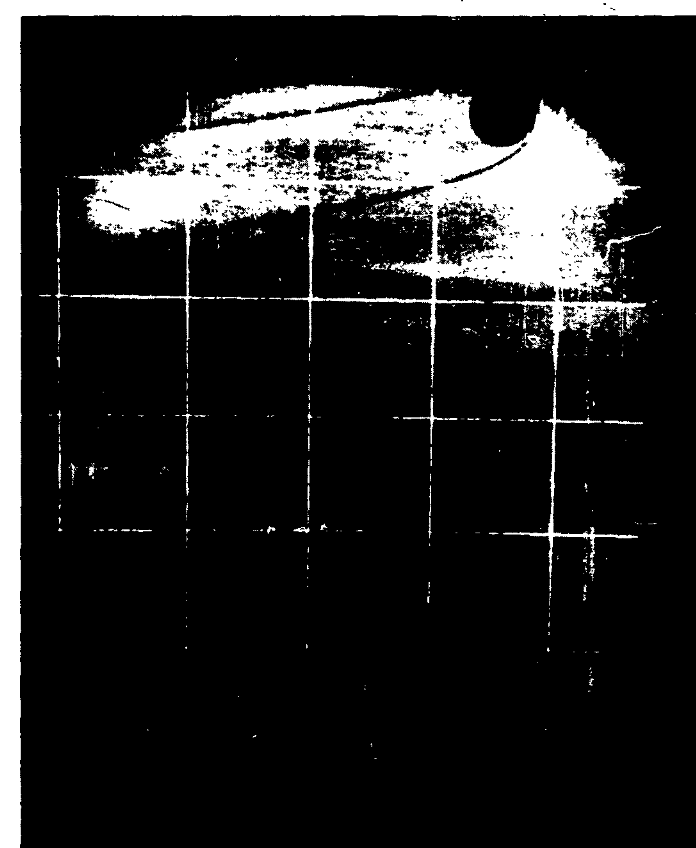


Fig. 7 Single High-Speed Camera Frame Showing Cones And A Sphere

Fig. 8 Composite Of 6 High-Speed Camera Frames

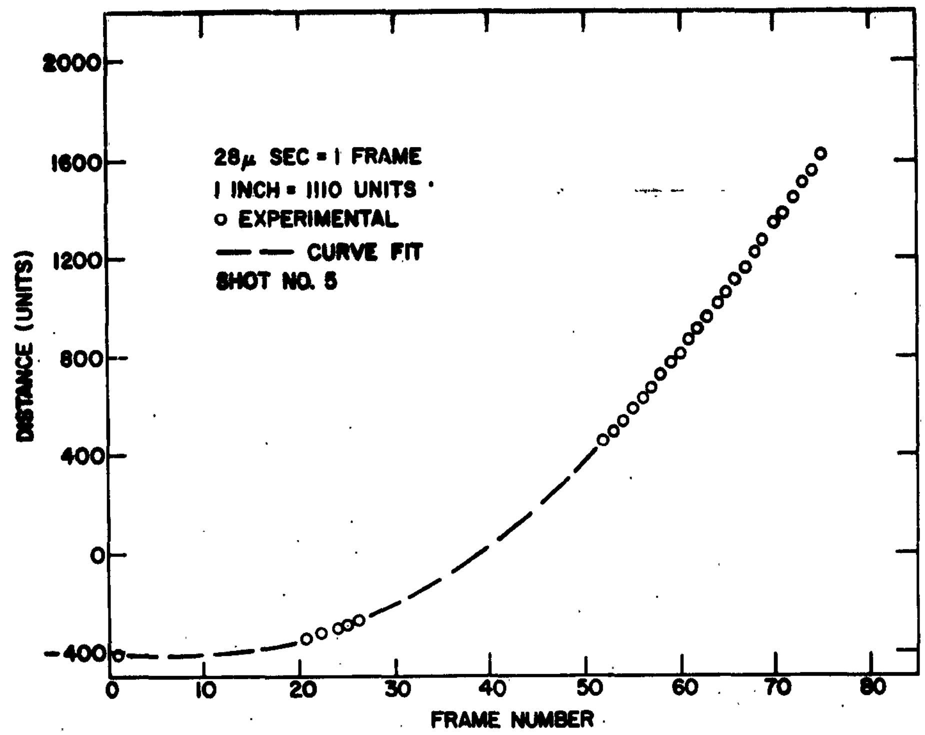


Fig. 9 Sphere Travel In 4-In. Shock Tunnel

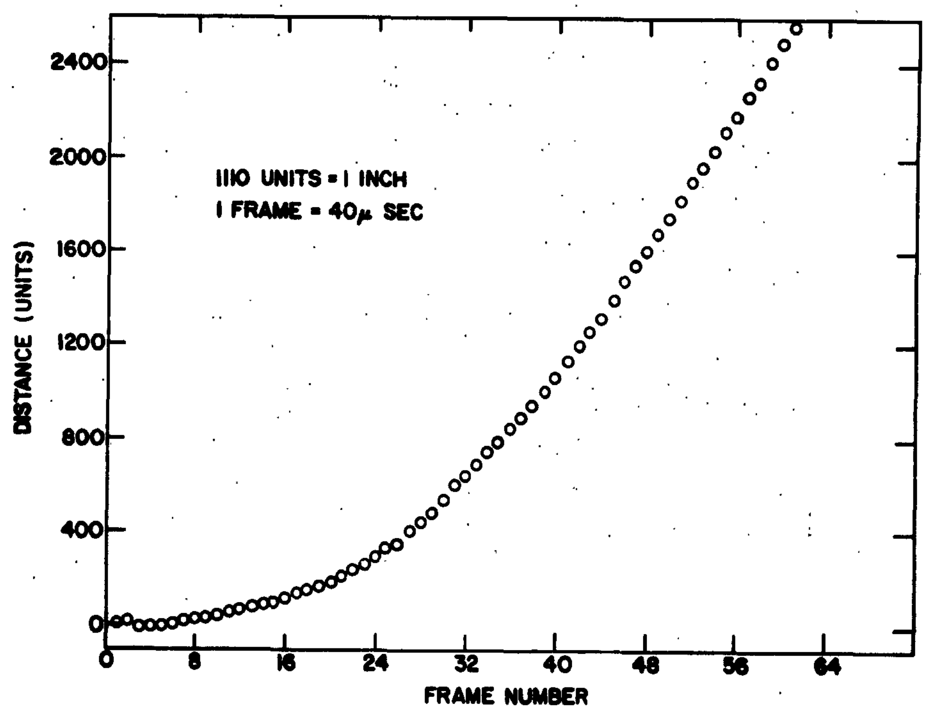


Fig. 10 Sphere Travel In 4-In. Shock Tunnel

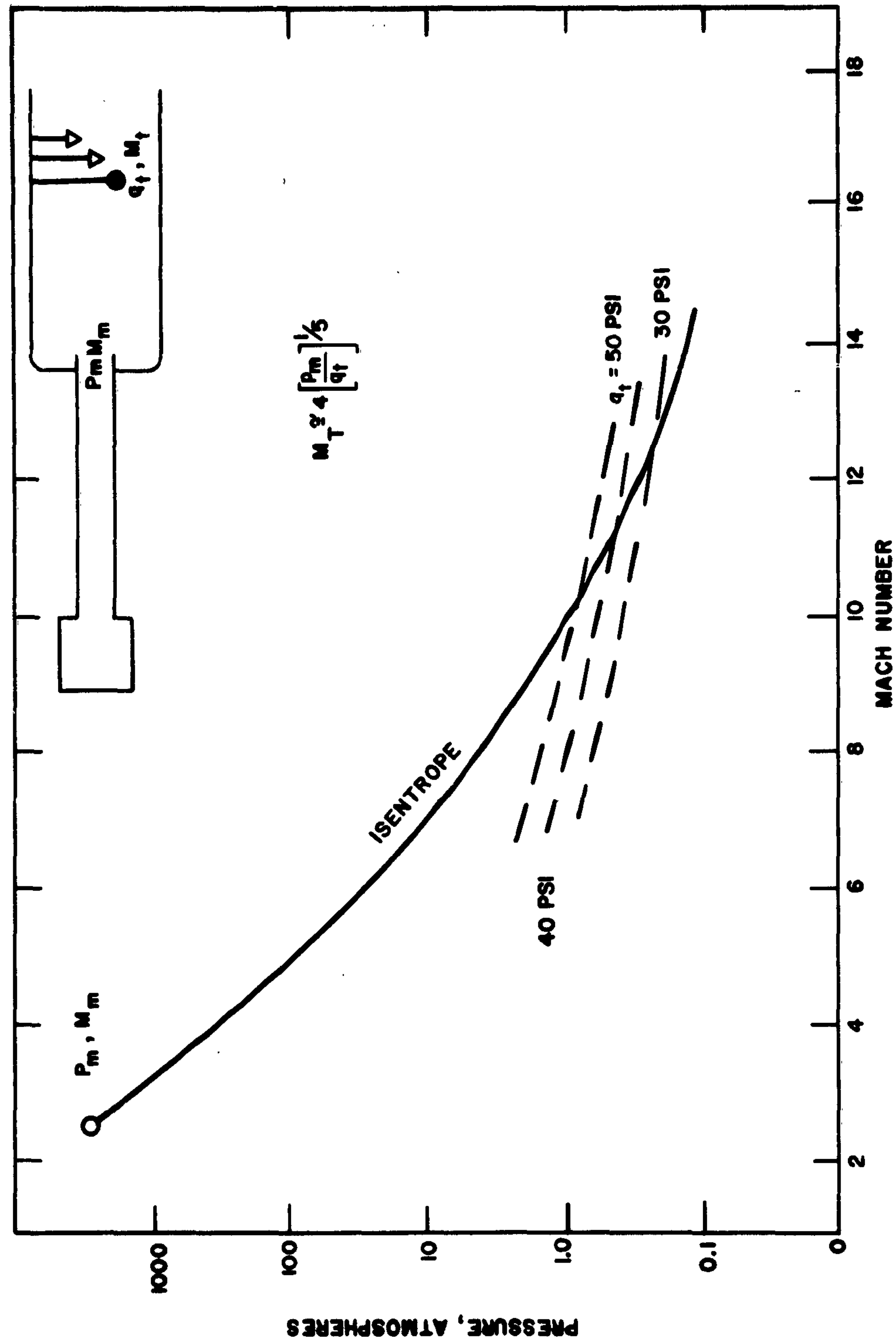


Fig. 11 Illustration Of Method Of Obtaining Test Section Mach Number

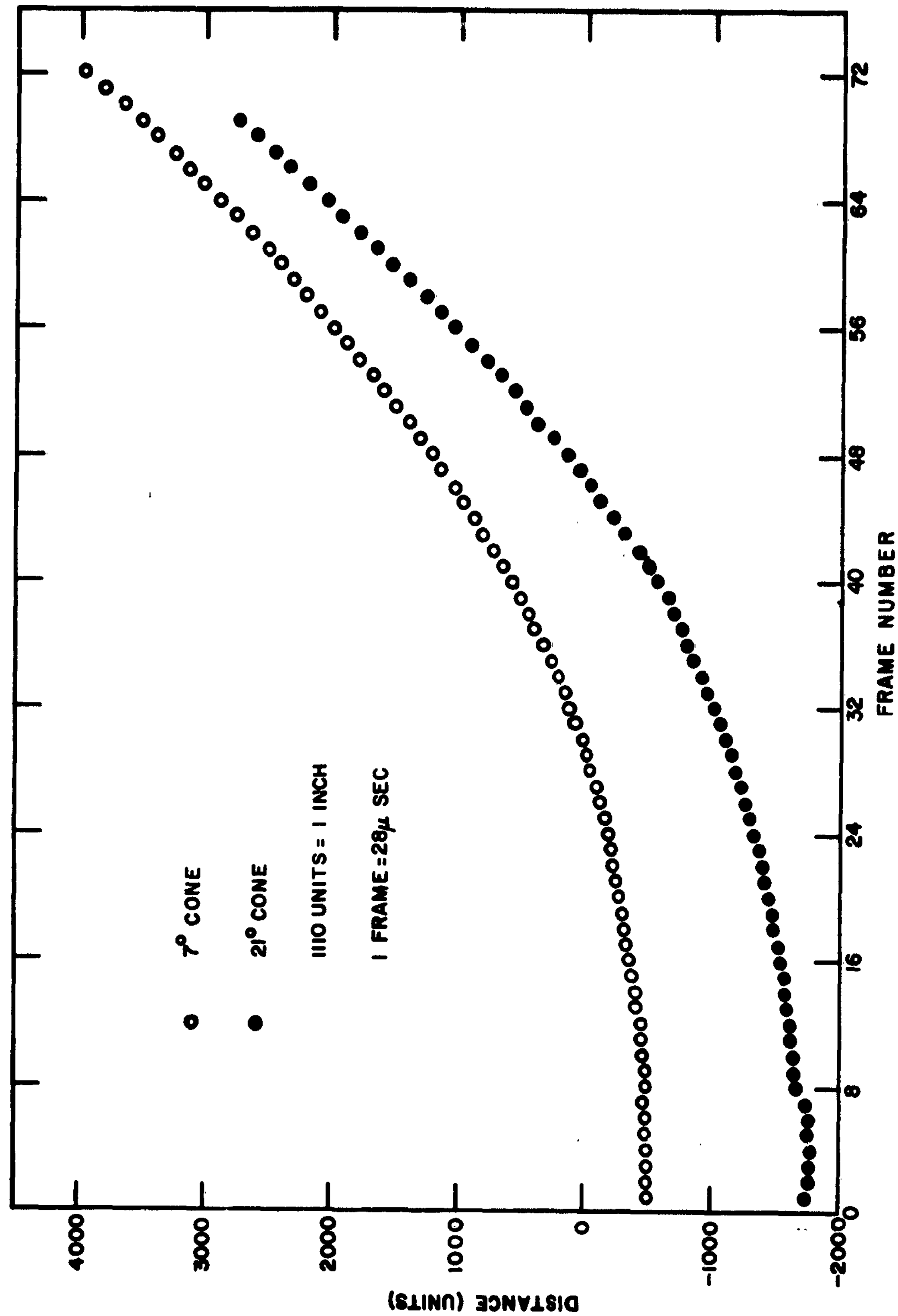


Fig. 12 Cone Drag Data

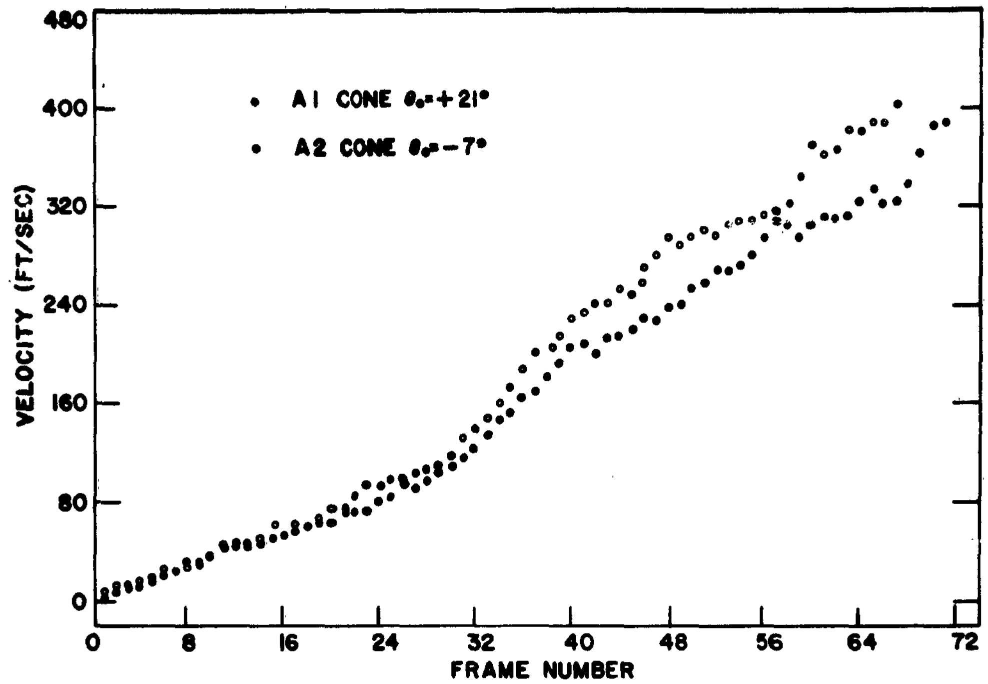


Fig. 13 Cone Velocity In Shock Tunnel

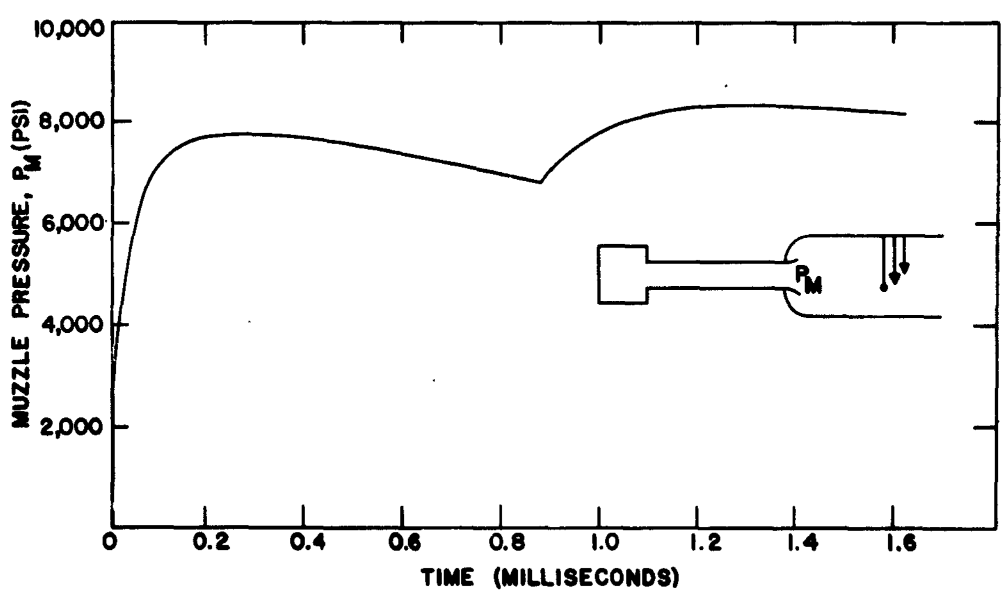


Fig. 14 Muzzle Pressure Versus Time Plot

WHERE OBTAINED	MACH NO.	$\frac{C_D \text{ (CONE)}}{C_D \text{ (SPHERE)}}$
OUR SHOCK TUBE WIND TUNNEL EXPERIMENT	10.5	1.63
BALLISTIC RANGE	6.85	1.57
INVISCID THEORY	10.5	1.67

Fig. 15 C_D Comparison Θ_{Max} 7°

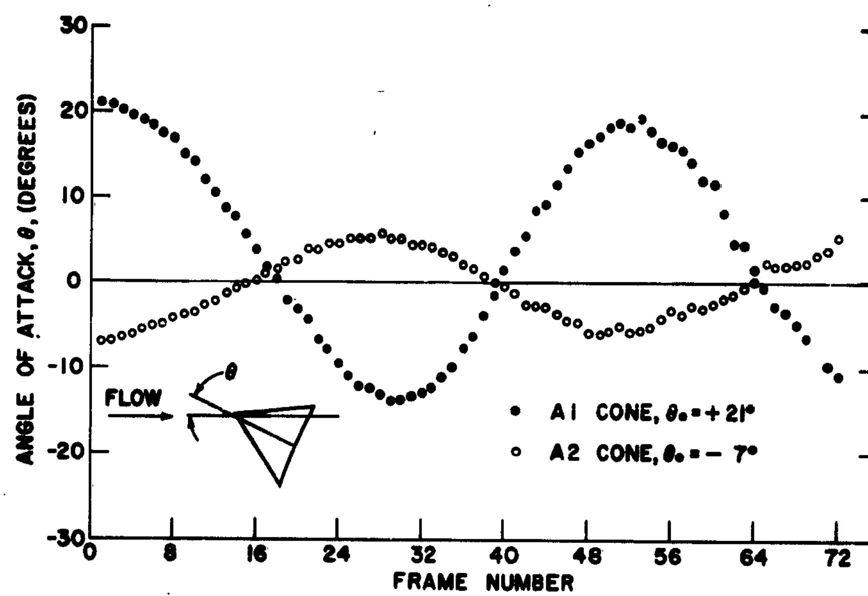


Fig. 16 Cone Stability Data

WHERE OBTAINED	MACH NO.	C_{M_α}
OUR SHOCK TUBE WIND TUNNEL EXPERIMENT	10.5	0.35
BALLISTIC RANGE	6.85	0.360
WIND TUNNEL	6.74	0.352

Fig. 17 C_M Comparison θ_{Max} 7°

CALCULATED QUANTITY	DATA USED
q	SPHERE MOTION
M	q AND MUZZLE PRESSURE
$\frac{C_D \text{ (CONE)}}{C_D \text{ (SPHERE)}}$	CONE MOTION
C_{M_α} (CONE)	CONE OSCILLATION FREQUENCY
$C_{M_q} + C_{M_{\dot\alpha}}$	CONE AMPLITUDE DECREMENT

Fig. 18 Data Obtainable In An Experiment Described

MATHEMATICAL ASPECTS OF SUPERSONIC NOZZLE DESIGN

by

Dr. Wm. R. Thickstun, Jr.
Chief, Mathematical Analysis Division
U. S. Naval Ordnance Laboratory

In spite of the title of my paper, Mathematical Aspects, I suppose most of what I shall present will be of more interest to our facility people. We have been called upon over the past ten years to calculate nozzle contours for the Naval Ordnance Laboratory and I feel that significant progress has been made. Fig. 1 shows a comparison of one of our newer nozzles with an old Mach 2.74 nozzle. The latter is actually a slightly modified Mach 2.48 nozzle, i.e. one of our old plaster type nozzles. It is a typical photograph with a large number of Mach lines or disturbances from the walls of the nozzle. Our new Mach 2.75, (actually a 2.76 nozzle) under somewhat similar operating conditions shows this picture: The attached shock on the cone model is quite visible but you see very little else. I might mention that the schlieren has not been exaggerated here in one case or the other. The intensity is substantially the same. We feel proud of this improvement in flow.

I would like to discuss the reason to which this improvement should be attributed. Most noteworthy is the improvement in construction technique. A second major change over the past ten years has been the use of high speed electronic computers to perform the calculations. This has made possible the use of a finer characteristic mesh and also the running of many more cases so as to meet all design requirements and compromises. Next I would like to mention the method of calculation. There is nothing special or novel here in that we have applied principles that have long been well-known and utilized in nozzle design. However, we have consistently and persistently, I might say, striven to obtain the most out of the calculation methods that we have used. Our approach has been to prescribe the flow desired by giving the Mach number distribution along the center line and then to calculate the nozzle contour that will produce this flow, rather than the converse problem. Fig. 2 shows a typical example: This shows the center line distribution for Mach number as a function of axial distance, a rather simple formula. This was for the Hypersonic Tunnel No. 8, that you may have seen yesterday. It gives a rapid rise from Mach 1 up to Mach 8 in this case. The computed nozzle contour is shown down below. Calculation proceeds by a straightforward method of characteristics from the center line to the nozzle wall.

There are facts that should be mentioned with respect to the Mach number distribution: one regarding continuity, the other regarding the region below Mach 1. It is well known that if the Mach number distribution be a function with a continuous n minus first derivative and a jump in the nth derivative, then there will be a jump in the n plus first derivative on the wall of the nozzle. So in particular, with this formula, we have gone into the uniform flow region with continuous slope and hence obtain continuous curvature on the wall of the nozzle. For flexible nozzle designs, one can conceivably demand of this function that a center line distribution be chosen which has continuous second derivative and hence obtain continuous third derivative on the wall. In this picture nothing has been specified below Mach 1 for the obvious reason that the method of characteristics cannot be used in the subsonic case. The usual thing is to make some assumption such as assuming a straight sonic line or perhaps assuming radial flow to exist, in order to get the computation started. Sometimes we have used the method of Nilson and Friedrichs, (a method of power series expansion in the stream function) and computed with the power series the flow back into the subsonic and through the throat. With such a procedure we can then pick up with the method of characteristics at the point L, shown in Fig. 3, and there is a region of overlap where the two methods yield the same wall contour. As always with power series methods, one eventually reaches a region of non-convergence, and in this case it appears in the supersonic. (The contour computed by the power series begins to wander quite widely as you go too far into the supersonic.) One difficulty encountered with this is that as we went to lower Mach numbers we found not a region of overlap but a gap between where the power series was satisfactory and where the method of characteristics could be used.

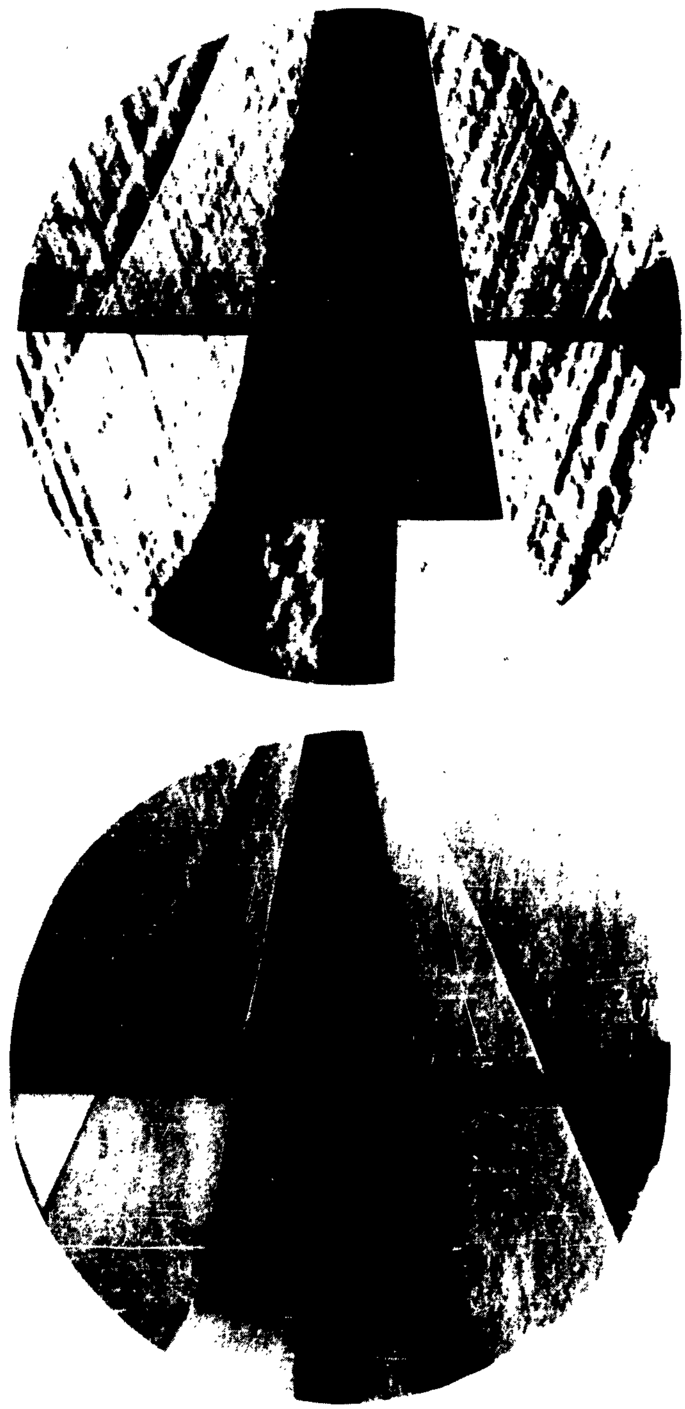
Fig. 4 shows a little more detail of the state of affairs in the throat region of the nozzle. This point L is where the characteristic that touches the sonic line on the axis meets the wall. If a center line be prescribed from the center line 0 through the supersonic, then with the method of characteristics the flow throughout this region to the right of L can be calculated and the nozzle contour obtained from L on out to the exit. The characteristics in the region between the sonic line and the characteristic OL are in a certain sense inaccessible directly and the question is, what sort of extension should be put on in the subsonic. At this point I would like to make a conjecture which is not completely proved at the moment but I will outline the direction of a probable proof. It is to be noted that if the center line distribution all the way from I on through to the supersonic be an analytic function, then in principle (whether you can calculate it or not) this flow including the contour is determined. So in particular, if one takes this function and calculates the contour LZ in the supersonic, the proper analytic continuation of the same contour that goes with the Mach number distribution that was specified would in principle be determined by finding (once the shape of this curbe LZ is found) the first derivative, the second, the third, the fourth and the remaining derivatives at the point L and then continuing this curve on back into the subsonic. One may or may not want to use this, assuming that it can be found. If there be a discontinuity at 0 in the center line distribution and hence along 0L, and we fit something else on here, the usual patching principle still applies along the characteristic 0L and it is possible, in principle, to continue with some other function having, say, the same second derivative at 0 and hence along 0L on back. So one may make the following conjecture: If the contour be continued with a "sufficiently gradual" curve into the subsonic, meeting at the point L with continuous first and second derivatives, then there will exist a continuous subsonic continuation of the flow without singularities. Clearly what is called for here in the way of rigorous proof is to show that any proposed continuation of the given contour LS, must satisfy certain inequalities which guarantee that there will be no singularities in the subsonic. Without such a rigorous proof, one can fair in a smooth curve and hope. In case of the high Mach number nozzles this is in fact precisely what we did successfully. At least it is as reasonable an assumption as to assume you have radial flow when you do not, or to assume you have a straight sonic line when you do not.

So much for calculations of the potential flow. This still does not furnish a contour for a finished nozzle. This brings us next to the question of boundary layer calculations, particularly at the higher Mach numbers where the boundary layers become so thick. A procedure is being used at NOL for calculating the turbulent boundary layer in the throat and the supersonic portion of the nozzle. The computed displacement thickness, δ^*, is then added to the height of the potential contour to obtain the final contour. Figure 5 shows a typical example. This is for Hypersonic Tunnel No. 4 but it is not completely shown here. The continuation would be on out to the right of the figure, but it does serve to illustrate the type of boundary growth that you can expect in the throat region, and as a matter of fact for the smaller radius of curvature in the throat of two inches, the growth of the boundary layer is fairly linear. For fixed nozzle blocks in two-dimensional nozzles, this is an advantage because it means that one can get away with corrections for boundary layer by tilting the blocks. In the case of axially symmetric nozzles we simply put this correction in. We have enough faith in this, I might add, that we definitely decided from various considerations to rely upon fixed nozzle blocks rather than to face the expense and problems of flexible nozzles. We feel that we have been successful in standardizing on the fixed nozzle blocks, even in the axial case where they are not movable. In the two-dimensional case, it should also be mentioned that calculations of the boundary layer have been made on the side walls so that these could be tapered to account, in part, for the boundary layer correction.

At this stage we still do not have a finished nozzle. We do have a finished contour calculation. In keeping with our principle of using the high speed computing machines and sort of leaving the calculations untouched by human hands, we have gone one step further and attempted to give the machine shop actual coordinates which they can use in machining the nozzle. By using the known radius of the cutter, and doing the simple calculation of finding the center of the cutter, and by doing an interpolation to obtain equal increments it is possible to furnish a complete set of cutter coordinate settings from which either a nozzle block can be cut directly, or from which in the axially symmetric case, a template can be cut from which a mandrel can be made. (See Figure 6) This leaves the shops in the position where, after taking many cuts, a nozzle can be made within a ten-thousandth of an inch of the computed contour by a final hand polishing.

In the axially symmetric case a mandrel was fabricated. Figure 7 shows the mandrel that was actually used for the Mach 10 nozzle for Hypersonic Tunnel No. 8. This has a 24-inch exit section -- you cannot tell the scale otherwise, and it appears foreshortened considerably from the position in which the photograph was taken. The throat, of course, is quite small. Finally, Figure 8 shows our Mach 2.75 two-dimensional nozzle again. This shows the pitot distribution down the center line; the throat is on the right and the nozzle box exit is marked on the figure. As usual, you have the pressure dropping, and you can hardly see it on the scale. This is an actual copy of the trace that was made. It does bulge a little below the line in one place, there is a little dip in another place, and it rises up a little bit in another, easily within 1 per cent error in pressure ratio. The other plot is for no particular nozzle but just meant to show the 5 per cent variation typical of some of the plaster nozzles in the past.

In closing, I might make a few more remarks. With our IBM 704 Computer, it is possible to run off a complete nozzle, even an axially symmetric one, in about five minutes computation, with another five minutes for boundary layer correction, making ten minutes altogether, so it would almost seem that we have reached the ultimate in nozzle design. However, this is still not true. At the present time we are working on extending nozzle design to real gases using data such as is obtainable from the Mollier charts to find $dp/d\rho$ and also doing the computation of real gas boundary layers; the thought being that we can actually design nozzles correctly for the real gas case.



a. 15 MICRO-INCH FINISH
$Ma_\infty = 2.76$ $Re_{\theta,C} = 2470$
Re/FT 3.62×10^6

b. 125 MICRO-INCH FINISH
$Ma_\infty = 2.74$ (MODIFIED 2.48) $Re_{\theta,C} = 3090$
Re/FT $= 3.63 \times 10^6$

Fig. 1 Spark Schlieren Photographs Of the 10° Sharp Cone

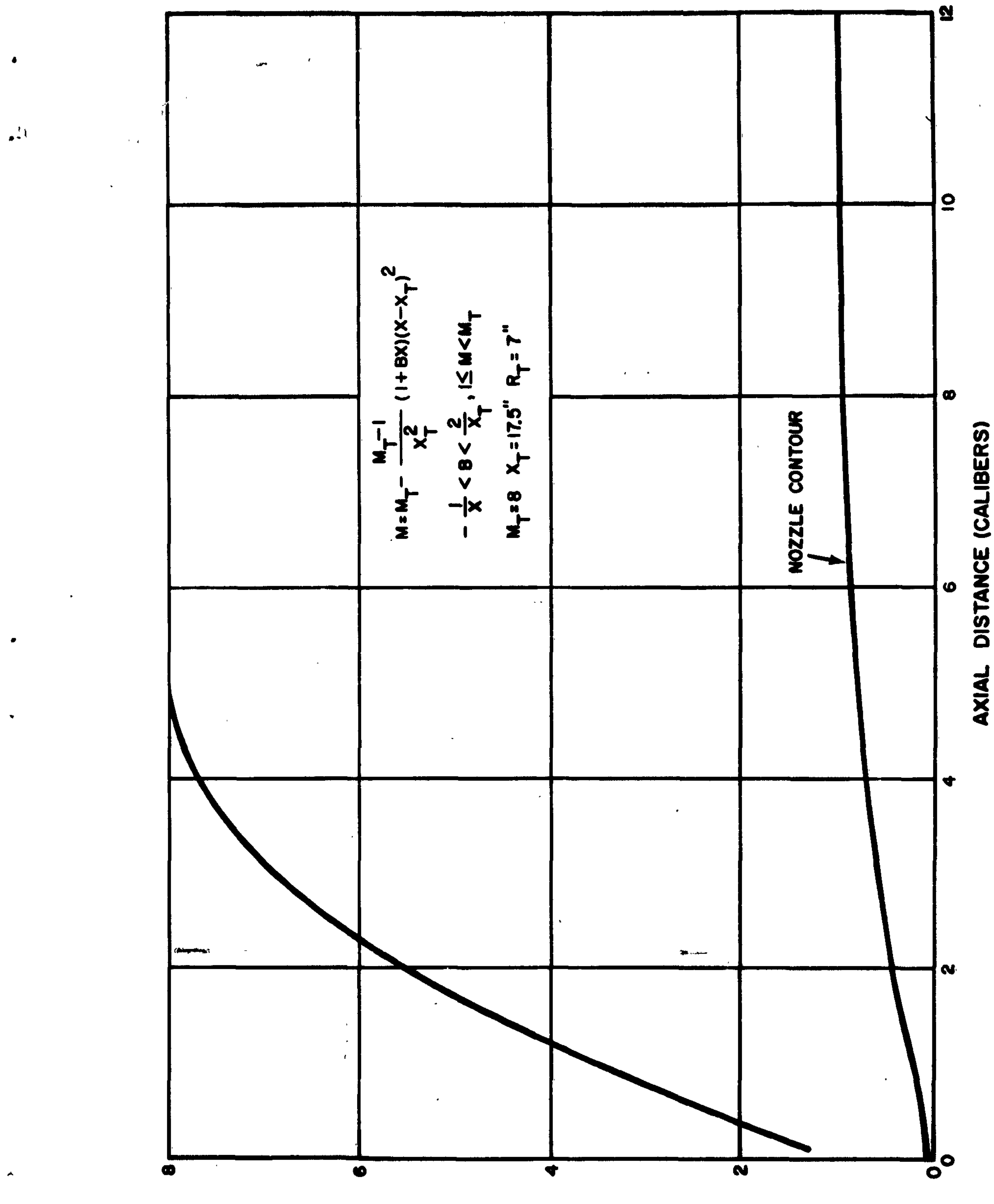


Fig. 2 Center Line Mach Number Distribution

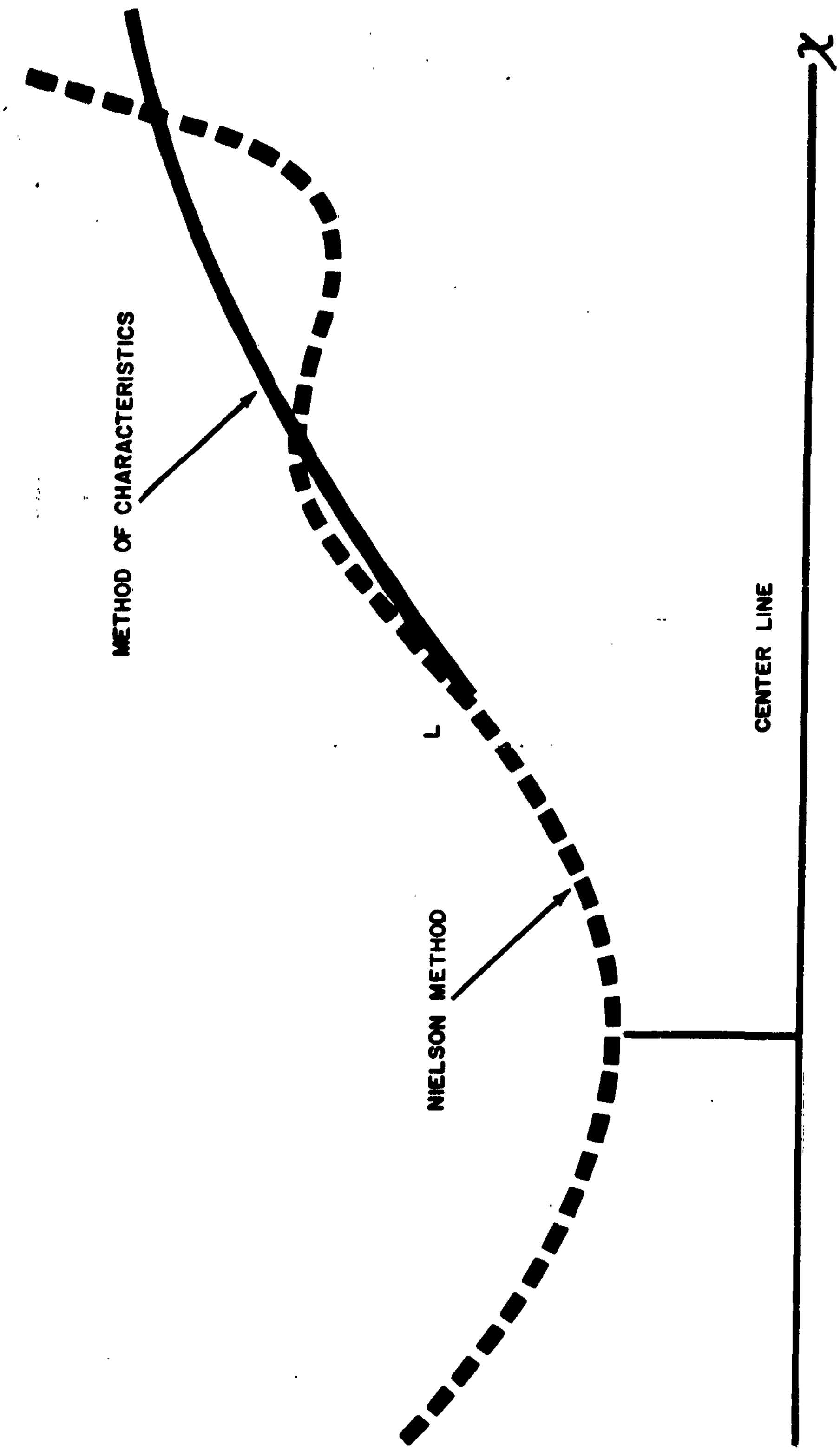


Fig. 3 Comparison Of Methods Of Nilson And Characteristics

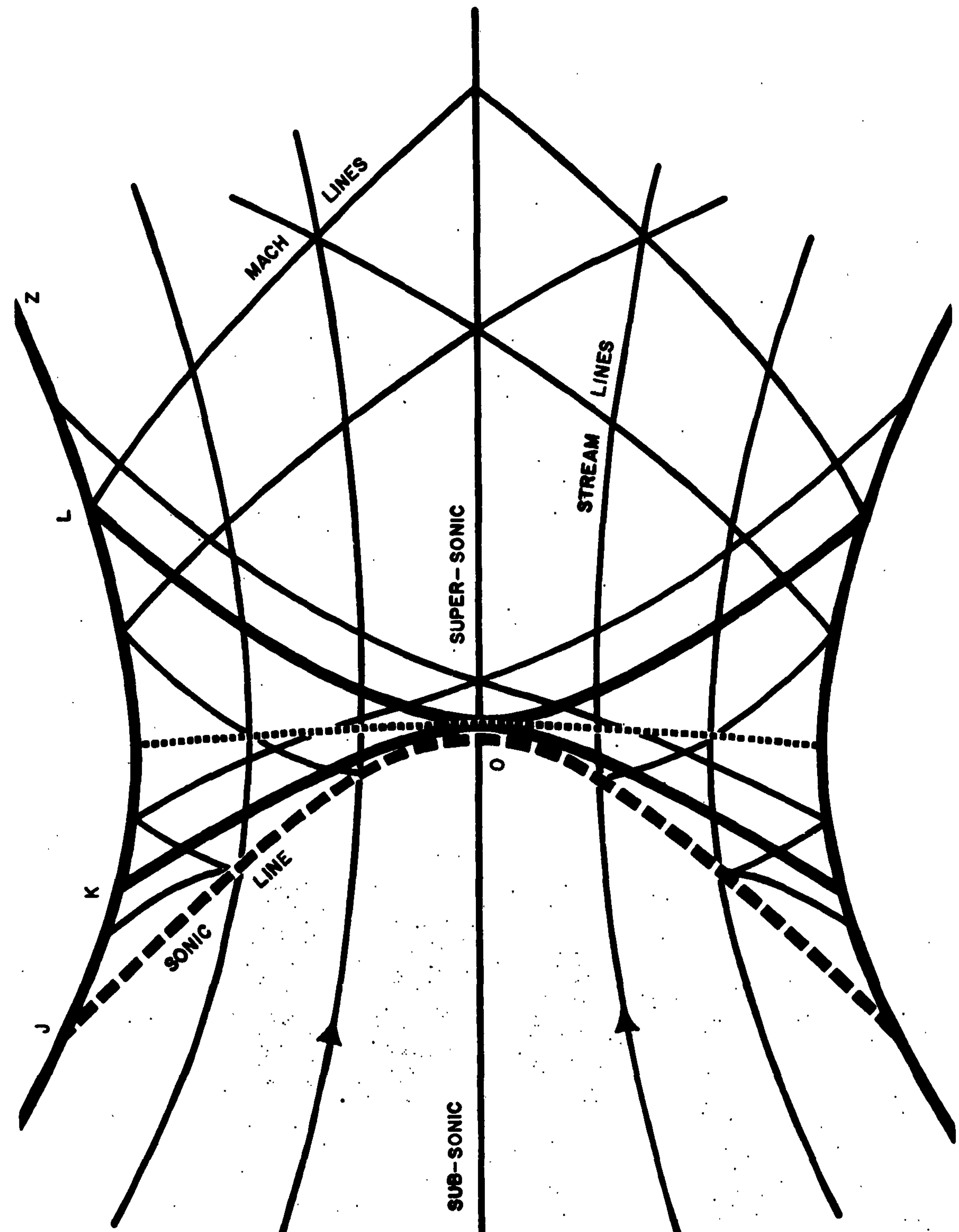


Fig. 4 Flow In Nozzle Throat

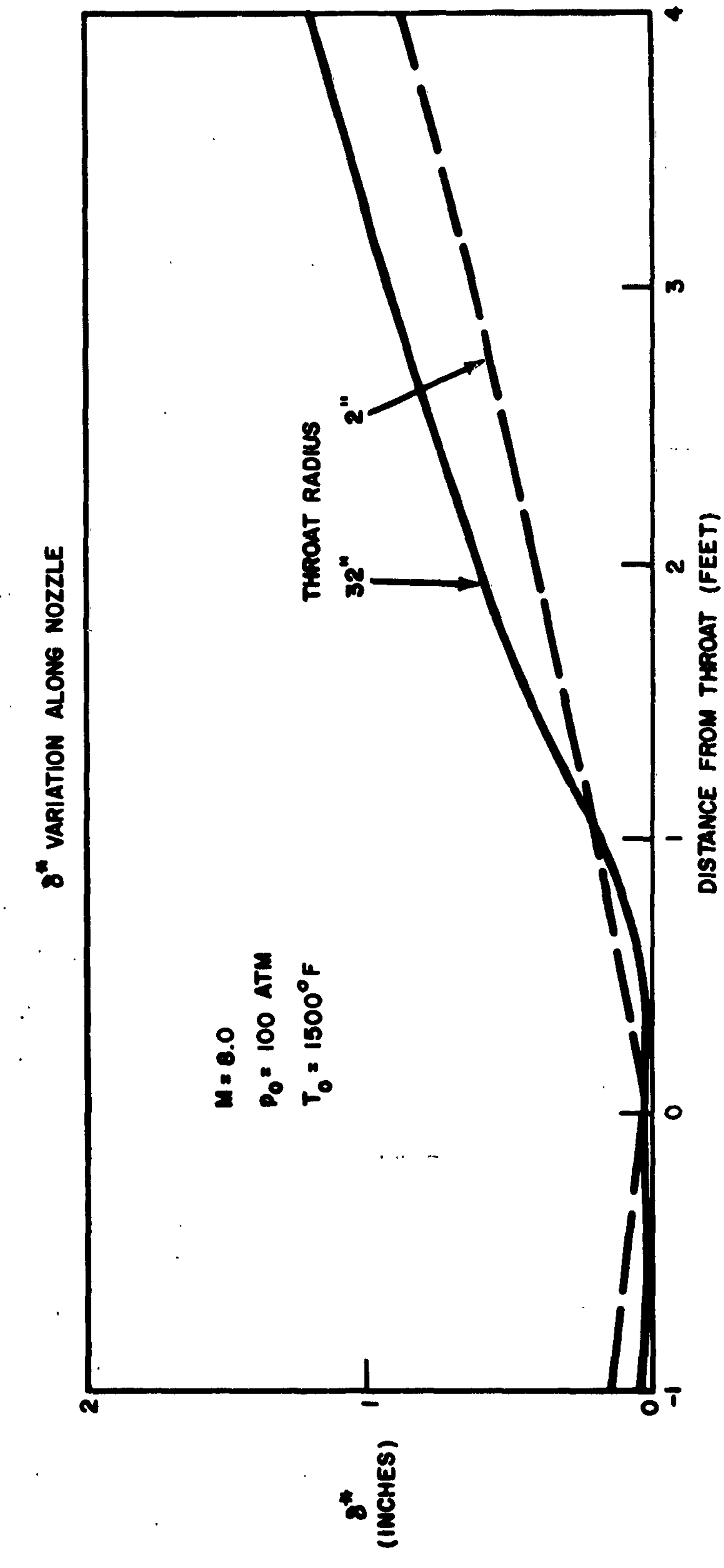


Fig. 5 Initial Boundary Layer Growth

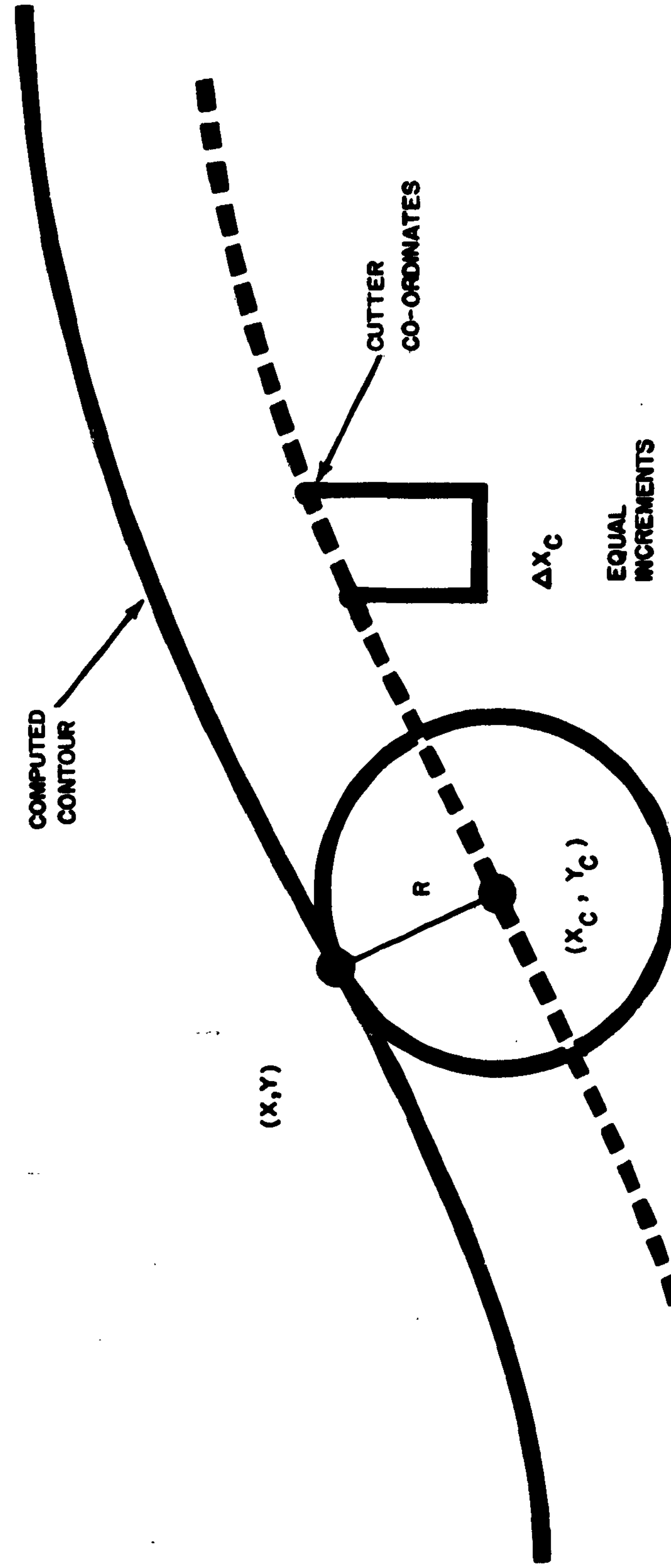


Fig. 6 Cutter Coordinates

Fig. 7 Mandrel For The Tunnel No. 8 Nozzle

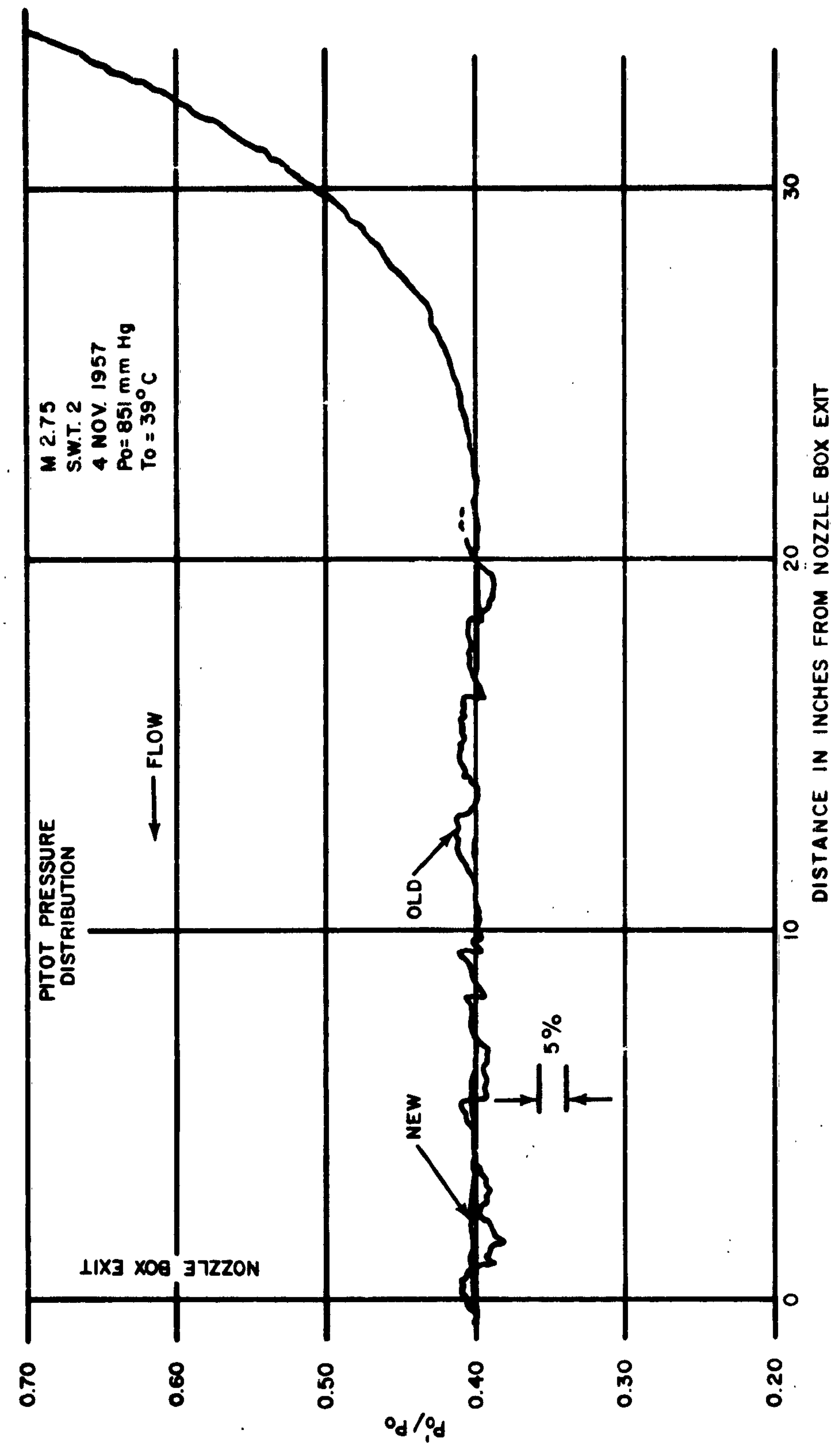


Fig. 8 Pitot Distributions - Old And New Nozzle

APPENDIX A

ROSTER OF PERSONS WHO REGISTERED AT THE U. S. NAVAL ORDNANCE LABORATORY FOR THE AEROBALLISTIC RESEARCH FACILITIES DEDICATION - DECENNIAL SYMPOSIUM

25 - 26 May 1959

Name	Affiliation
Ablard, J. E., Dr.	U. S. Naval Ordnance Laboratory
Adams, W. I.	U. S. Naval Ordnance Laboratory
Akerman, John D., Prof.	University of Minnesota
Allen, Paul H.	The Ralph M. Parsons Company
Alred, R. V.	British Joint Services Mission
Anderson, Carl C., Capt.	Air Force Institute of Technology
Andryshak, R.	U. S. Naval Ordnance Laboratory
Ansley, Sterling	AMRAD Corporation
Applebaum, A.	U. S. Naval Ordnance Laboratory
Armstrong, J. H.	U. S. Naval Ordnance Laboratory
Auld, C. D.	U. S. Naval Ordnance Laboratory
Baker, Judd O.	Bureau of Ships
Baker, W. K.	Bureau of Ordnance
Beach, E. H., Dr.	U. S. Naval Ordnance Laboratory
Beaman, H. C., Col. (Ret.)	Air Research & Development Command
Beard, M.	U. S. Naval Ordnance Laboratory
Bell, G. L., Cdr. (SC) (USN)	U. S. Naval Air Station, Glenview, Ill.
Belliveau, L. J.	U. S. Naval Ordnance Laboratory
Benham, F. S.	U. S. Naval Ordnance Laboratory
Berg, F. H., Lt.	University of Maryland
Bird, K. D.	Cornell Aeronautical Laboratory, Inc.
Birmingham, A. N.	U. S. Naval Ordnance Laboratory
Bleil, D. F., Dr.	U. S. Naval Ordnance Laboratory
Bogle, Robert W.	
Bowen, G. C.	U. S. Naval Ordnance Laboratory
Bowen, J. I., Dr.	U. S. Naval Ordnance Laboratory
Bratt, Robert W.	Douglas Aircraft Co., Inc., El Segundo
Brehm, J. J.	U. S. Naval Ordnance Laboratory
Brodsky, R. F.	Aerojet General Corporation, Azusa
Brooke, E.	U. S. Naval Ordnance Laboratory

Brown, F. W.	U. S. Naval Ordnance Laboratory
Brown, G. A., Prof.	Massachusetts Institute of Technology
Brown, L.	U. S. Naval Ordnance Laboratory
Burgers, J. M., Prof.	University of Maryland
Burington, R. S., Dr.	Bureau of Ordnance
Burnett, F.	Bureau of Ordnance
Callen, E.	U. S. Naval Ordnance Laboratory
Campbell, G., Dr.	Rensselaer Polytechnic Institute
Campbell, R.D., Jr., Lt. Col.	Arnold Engineering Development Center
Cerreta, P.	U. S. Naval Ordnance Laboratory
Charters, A. C., Dr.	Ames Research Center, NASA
Chones, A. J.	U. S. Naval Ordnance Laboratory
Clark, C. W., Brig. Gen.	U. S. Army Ordnance
Clark, Austin B. J.	Naval Research Laboratory
Clark, E. C.	Crane Company
Collins, D.	U. S. Naval Ordnance Laboratory
Compher, F., Mrs.	U. S. Naval Ordnance Laboratory
Conlan, J.	U. S. Naval Ordnance Laboratory
Cooper, L. G.	The Martin Company
Cornett, R.	U. S. Naval Ordnance Laboratory
Corning, Gerald, Prof.	University of Maryland
Coski, G. S.	U. S. Naval Ordnance Laboratory
Cronvich, L. L.	Applied Physics Laboratory, JHU
Czajkowski, N.	U. S. Naval Ordnance Laboratory
Dahl, R.	Aerojet General Corp., Wash. D. C.
Danberg, J.	U. S. Naval Ordnance Laboratory
Davis, Stephen S., Prof.	Howard University
De Los Santos, S., Dr.	David Taylor Model Basin
~~DeMeritte, F.~~	U. S. Naval Ordnance Laboratory
Dexter, Robert R.	Institute of the Aeronautical Sciences
Diaz, J., Dr.	U. S. Naval Ordnance Laboratory
Dinsmoor, T. E.	American Machine and Foundry Co.
Dodge, J., Col.	Ballistic Missile Div., ARDC, Inglewood
Doig, Leroy L., Jr.	Naval Ordnance Test Station, China Lake
Donoghue, F. D.	Bureau of Ordnance
Donovan, R.E.H., Jr., Capt.	Air Research & Development Command
Dornberger, Walter R., Dr.	Bell Aircraft Corporation
Dryden, H. L., Dr.	National Aeronautics & Space Admin.
Eckert, E.R.G., Dr.	University of Minnesota
Englert, G. W.	National Aeronautics & Space Admin.

Enkenhus, K. R.	U. S. Naval Ordnance Laboratory
Ennis, W. R., Cdr.	Bureau of Ordnance
Evans, H. F.	U. S. Naval Ordnance Laboratory
Falusi, M. E.	U. S. Naval Ordnance Laboratory
Farrall, J. C.	U. S. Naval Ordnance Laboratory
Featherstone, C.M., Jr.	AVCO Manufacturing Company
Fedenia, J. N.	U. S. Naval Ordnance Laboratory
Fedor, J., Dr.	U. S. Naval Ordnance Laboratory
Felix, A. R.	Army Ballistic Missile Agency
Filler, W. S.	U. S. Naval Ordnance Laboratory
Foa, Joseph V., Prof.	Rensselaer Polytechnic Institute
Fye, Paul, Dr.	Woods Hole Oceanographic Institute
Gaetano, F.	U. S. Naval Ordnance Laboratory
Galloway, H.	U. S. Naval Ordnance Laboratory
Gauzza, H.	U. S. Naval Ordnance Laboratory
Geineder, F.	U. S. Naval Ordnance Laboratory
Gibson, Ralph E., Dr.	Applied Physics Laboratory, JHU
Gilbert, B.	U. S. Naval Ordnance Laboratory
Gilliam, R. P.	Bureau of Ordnance
Gleason, D.	U. S. Naval Ordnance Laboratory
Gleyzal, A.	U. S. Naval Ordnance Laboratory
Glowacki, W. J.	U. S. Naval Ordnance Laboratory
Gorges, H. A.	Army Ballistic Missile Agency
Graham, Hylton	National Bureau of Standards
Grantham, R. E.	U. S. Naval Ordnance Laboratory
Greenwood, E. W.	Canadian Joint Staff
Green, Jerome B., Dr.	Johns Hopkins University
Greene, J.	U. S. Naval Ordnance Laboratory
Griffin, T. F.	Bureau of Ordnance
Grimmer, D. I.	U. S. Naval Ordnance Laboratory
Gunkel, Robert J.	Douglas Aircraft Company, Inc.
Hale, R. E.	ARDC, Andrews Air Force Base
Harkelroad, N.E., Capt. USN	David Taylor Model Basin
Harris, E. L., Dr.	U. S. Naval Ordnance Laboratory
Harrison, W. K., Jr.	U. S. Naval Ordnance Laboratory
Hartert, J. F.	U. S. Naval Ordnance Laboratory
Hartmann, G. K., Dr.	U. S. Naval Ordnance Laboratory
Haseltine, W. R., Dr.	Naval Ordnance Test Station, China Lake
Haynes, E.	Air Force Office of Scientific Research
Hayward, J. T., Vice Adm. USN	Office of Chief of Naval Operations
Hayward, R., Brig. Gen. (Ret.)	Aerojet General Corporation

Heinrich, Helmut G., Dr.	University of Minnesota
Hermann, Rudolph, Prof.	University of Minnesota
Herzfeld, K. F., Prof.	Catholic University
Highdale, J.	U. S. Naval Ordnance Laboratory
Hoadley, Henry H.	United Aircraft Corporation
Hoffman, J. W.	U. S. Naval Ordnance Laboratory
Hollingsworth, R. T.	U. S. Naval Ordnance Laboratory
Holt, J. E.	U. S. Naval Ordnance Laboratory
Hoppin, Glenn (Ret.)	
Horanoff, E. V.	U. S. Naval Ordnance Laboratory
Howard, D. E., Prof.	Howard University
Hubbard, B. E.	U. S. Naval Ordnance Laboratory
Hubbard, Miles H., Rear Adm.	Bureau of Ordnance
Hunt, E. L.	U. S. Naval Ordnance Laboratory
Iandolo, J.	U. S. Naval Ordnance Laboratory
Jackson, A.	U. S. Naval Ordnance Laboratory
Jacobs, S. G., Dr.	U. S. Naval Ordnance Laboratory
Johnstone, C. A.	Bureau of Ordnance
Jones, G. C.	U. S. Naval Ordnance Laboratory
Jones, S.	U. S. Naval Ordnance Laboratory
Kabik, I.	U. S. Naval Ordnance Laboratory
Kahne, G. F.	U. S. Naval Ordnance Laboratory
Kamm, R. W.	Arnold Engineering Development Center
Kaplan, L.	U. S. Naval Ordnance Laboratory
Karpov, B. G., Dr.	Ballistic Research Laboratory, Aberdeen
Kartalis, A.	Bureau of Docks
Katz, J.	U. S. Naval Ordnance Laboratory
Kelly, M.R., Rear Adm.(Ret.)	Applied Physics Laboratory, JHU
Kendall, J. M.	U. S. Naval Ordnance Laboratory
Kendig, Paul, Dr.	Pennsylvania State University
Kerstetter, D.	U. S. Naval Ordnance Laboratory
Kirby, R.	General Electric Company
Korobkin, I.	U. S. Naval Ordnance Laboratory
Krahn, Dorothee	U. S. Naval Ordnance Laboratory
Kuhlthau, A. R., Dr.	University of Virginia
Kurzweg, H. H., Dr.	U. S. Naval Ordnance Laboratory
Lampson, C. W., Dr.	Ballistic Research Laboratory, Aberdeen
Lane, D. C.	U. S. Naval Ordnance Laboratory

Langenbeck, E. H.	U. S. Naval Ordnance Laboratory
Lankford, J. L.	U. S. Naval Ordnance Laboratory
Larsson, C. E.	Flygmoter Aero.Eng. Co., Sweden
Leadon, Bernard M., Dr.	Convair
Lee, John D., Dr.	Ohio State University
Lee, J. M.	Bureau of Ordnance
Lee, R. E.	U. S. Naval Ordnance Laboratory
Lehnert, R., Dr.	U. S. Naval Ordnance Laboratory
Levensteins, Z. I.	U. S. Naval Ordnance Laboratory
Li, Ting Yi	Rensselaer Polytechnic Institute
Liberman, E.	U. S. Naval Ordnance Laboratory
Lightbody, A., Dr.	U. S. Naval Ordnance Laboratory
Lightfoot, J. R.	U. S. Naval Ordnance Laboratory
Lightner, T.A.,Cdr.(SC)(USN)	Naval Air Station, Pensacola
Lobb, R. K., Dr.	U. S. Naval Ordnance Laboratory
Long, Joseph E.	Air Force Office of Scientific Research
Lord, D. E.	U. S. Naval Ordnance Laboratory
Ludford, G., Dr.	University of Maryland
Lukasiewicz, J.	Arnold Research Org., Inc. Tullahoma
MacCloskey, M., Brig. Gen.	Crosley Division, AVCO
Maechler, Gerald C.	Nordberg Company
Maher, E. F.	U. S. Naval Ordnance Laboratory
Martin, John, Dr.	Bendix Products Div., South Bend, Ind.
Martin, Monroe H., Prof.	University of Maryland
Martini, P. J.	U. S. Naval Ordnance Laboratory
McDermitt, W. N.	Arnold Research Organization, Inc.
McGuire, T. R., Dr.	U. S. Naval Ordnance Laboratory
McKay, William L.	AVCO Manufacturing Corporation
McMillen, Howard J., Dr.	National Science Foundation
Metcalf, George	General Electric Company
Miller, Charles	U. S. Naval Ordnance Laboratory
Miller, Ralph	Boeing Airplane Company
Mills, F. B.	U. S. Naval Ordnance Laboratory
Milner, L.	U. S. Naval Ordnance Laboratory
Mirakian, P.	Bureau of Ordnance
Morkovin, Mark V.	The Martin Company
Morrison, T. D.	U. S. Naval Ordnance Laboratory
Morton, Wilbur Y.	Bureau of Ordnance
Mott-Smith, H. M., Dr.	Gibbs and Cox, Inc.
Muir, M.	U. S. Naval Ordnance Laboratory
Munk, M. M., Dr.	Catholic University
Murphy, Charles H., Dr.	Ballistic Research Laboratory, Aberdeen

Muzzey, D. S., Dr.	U. S. Naval Ordnance Laboratory
Myers, E. H.	Allis Chalmers Manufacturing Company
Nelson, Wilbur C., Prof.	University of Michigan
Nicholson, L. F.	Royal Aircraft Establishment, England
Nichter, F. N.	U. S. Naval Ordnance Laboratory
Nicolaides, J. D.	Bureau of Ordnance
Noonan, B.	U. S. Naval Ordnance Laboratory
Northam, J. C.	Worthington Corporation
O'Brien, E.	U. S. Naval Ordnance Laboratory
Ogan, R. A.	U. S. Naval Ordnance Laboratory
Orlik-Ruckemann, K.	National Aero. Estab., Canada
Orlin, James	AMRAD Corporation
Ostrom, D.Y., Jr., Lt.Col.	U. S. Army Ordnance
Paine, R. W., Jr., Capt.	Bureau of Ordnance
Pai, Shi-I, Dr.	University of Maryland
Parker, H. M., Dr.	University of Virginia
Pashby, P.	U. S. Naval Ordnance Laboratory
Pasiuk, L.	U. S. Naval Ordnance Laboratory
Patterson, Gordon N., Dr.	University of Toronto
Payne, G. W.	U. S. Naval Ordnance Laboratory
Pehrson, G. O.	Special Projects Office, BuOrd
Perry, H. A.	U. S. Naval Ordnance Laboratory
Persh, J.	Special Projects Office, BuOrd
Peschel, William	AMRAD Corporation
Peterson, A. C.	U. S. Naval Ordnance Laboratory
Peterson, M.A., Capt. USN	U. S. Naval Ordnance Laboratory
Peucker, M.	General Electric Company
Phillips, J.O., Jr., Capt. USN	Bureau of Ordnance
Pipitone, S. J.	Goodyear Aircraft Corporation
Pittman, C. W.	Bureau of Ordnance
Plotkin, G. N.	U. S. Naval Ordnance Laboratory
Polachek, Harry, Dr.	David Taylor Model Basin
Pooler, L.G., Cdr. USN(Ret.)	
Postle, R. S., Jr.	Bell Aircraft Corporation
Potts, J. H., Jr.	Special Projects Office, BuOrd
Power, R.B., Dr.	Ordnance Corps, Dept. of the Army
Powers, J.	U. S. Naval Ordnance Laboratory
Preisman, Albert	Capital Radio Engineering Institute
Prelowski, S.	PRESCO
Prescott, R.	AVCO Manufacturing Corporation

Quense', J.A., Capt. USN	U. S. Naval Ordnance Laboratory
Quinn, R. B.	Bureau of Ordnance
Raborn, W.F., Rear Adm.	Special Projects Office, BuOrd
Ramsay, B.P., Dr.	U. S. Naval Ordnance Laboratory
Ray, K. J.	Foster Wheeler Corporation
Reed, J. J.	Office of Assistant Secretary of Navy
Reed, J. V.	U. S. Naval Ordnance Laboratory
Reese, W. R., Lt.Cdr.	U. S. Naval Ordnance Laboratory
Reiss, H. R., Dr.	U. S. Naval Ordnance Laboratory
Richter, V. J.	U. S. Naval Ordnance Laboratory
Roberts, R. C., Dr.	U. S. Naval Ordnance Laboratory
Robertson, W. B., Cdr.	Bureau of Ordnance
Robertson, G. D.	Magnavox Co.
Rogers, Milton	Air Force Office of Scientific Research
Rointer, J. F.	Allis Chalmers Manufacturing Company
Ross, D. H.	Massachusetts Institute of Technology
Ross, Sol	Goodyear Aircraft Corporation
Ruckner, E. A., Capt.	Bureau of Ordnance
Santomieri, J.	Bendix Products Division, South Bend, Ind.
Sarelas, N. P.	U. S. Naval Ordnance Laboratory
Saunders, J.	U. S. Naval Ordnance Laboratory
Schindel, Leon H.	Massachusetts Institute of Technology
Schlesinger, M.	U. S. Naval Ordnance Laboratory
Schnackenberg, Fred	Nordberg Company
Schoeffel, M.F., Rear Adm.(Ret.)	General Precision Equip. Corp.
Schroth, R. T.	U. S. Naval Ordnance Laboratory
Schwartz, I. R.	Air Force Office of Scientific Research
Seeger, R. J., Dr.	National Science Foundation
Seeley, Walter J., Dean	Duke University
Seidman, O.	Bureau of Aeronautics
Seigel, A. E., Dr.	U. S. Naval Ordnance Laboratory
Shantz, I.	U. S. Naval Ordnance Laboratory
Shen, Shan Fu, Prof.	University of Maryland
Shepard, B. M.	U. S. Naval Ordnance Laboratory
Shortley, George, Dr.	Booz-Allen Applied Research, Inc.
Sibila, A. I.	Chance Vought Aircraft
Silvers, S.	American Machine & Foundry Company
Slawsky, M. M., Dr.	Air Force Office of Scientific Research
Slawsky, Z. I., Dr.	U. S. Naval Ordnance Laboratory
Smith, Levering, Capt.	Special Projects Office, BuOrd
Smith, R. J.	U. S. Naval Ordnance Laboratory
Smith, T., Dr.	Ballistic Research Laboratory, Aberdeen

Snavely, B. L., Dr.	U. S. Naval Ordnance Laboratory
Solem, A., Dr.	U. S. Naval Ordnance Laboratory
Soule, Harold V.	Waltham, Mass.
Spriggs, T.	U.S. Naval Ordnance Laboratory
Stair, J. R.	Goodyear Aircraft Corporation
Starbuck, F.	U. S. Naval Ordnance Laboratory
Steg, Leo, Dr.	General Electric Company
Steinle, W. C.	Bureau of Ordnance
Steinmetz, Harold F.	McDonnell Aircraft Corporation
Stengard, Edwin O.	Naval Gun Factory
Stephens, P.	U. S. Naval Ordnance Laboratory
Sternberg, Joseph	Ballistic Research Laboratory, Aberdeen
Stevens, C. J.	Bureau of Yards and Docks
Stevens, H. L.	U. S. Naval Ordnance Laboratory
Stoner, D. W.	Naval Proving Ground, Dahlgren
Stotz, R. J.	U. S. Naval Ordnance Laboratory
Stout, E. G.	The Ralph M. Parsons Company
Stretton, T. R.	British Joint Services Mission
Summers, C.	District Public Works Office
Terry, Joseph E.	Naval Ordnance Laboratory, Corona
Tetervin, N.	U. S. Naval Ordnance Laboratory
Thale, J. S., Dr.	Cook Research Laboratory
Theodorsen, T., Dr.	Republic Aviation Corporation
Thickstun, W. R., Dr.	U. S. Naval Ordnance Laboratory
Thole, J.	U. S. Naval Ordnance Laboratory
Thomas, Wiley, Jr.	University of Tennessee
Thurman, D.	U. S. Naval Ordnance Laboratory
Thurston, P. A.	U. S. Naval Ordnance Laboratory
Tickner, A. J.	Naval Ordnance Test Station, Pasadena
Timme, A. R., Dr.	U. S. Naval Ordnance Laboratory
Tipton, R.	U. S. Naval Ordnance Laboratory
Tulin, M. P.	Convair, San Diego
Tweney, G. H.	Boeing Airplane Company
Vas, I. E., Prof.	Princeton University
Vogt, C. C.	U. S. Naval Ordnance Laboratory
Volz, W.	U. S. Naval Ordnance Laboratory
von Braun, Wernher, Dr.	Army Ballistic Missile Agency
von Eschen, Garvin L., Prof.	Ohio State University
von Karman, Theodore, Dr.	AGARD
Wadman, A. J.	U. S. Naval Ordnance Laboratory
Walton, S.	U. S. Naval Ordnance Laboratory

Wasser, R. H.	U. S. Naval Ordnance Laboratory
Webb, J.	U. S. Naval Ordnance Laboratory
Weinstein, A., Prof.	University of Maryland
Welch, R. V., Cdr.	U. S. Naval Ordnance Laboratory
Wessel, R., Cdr.	U. S. Naval Ordnance Laboratory
West, E. P.	U. S. Naval Ordnance Laboratory
Westin, H. S.	U. S. Naval Ordnance Laboratory
White, C.	U. S. Naval Ordnance Laboratory
Wilcox, R. J.	Allis Chalmers Manufacturing Company
Wild, John M.	Arnold Research Organization
Wilkie, W. J.	U. S. Naval Ordnance Laboratory
Wilson, R. E., Dr.	U. S. Naval Ordnance Laboratory
Wineland, W. C., Dr.	U. S. Naval Ordnance Laboratory
Winkler, E. H., Dr.	U. S. Naval Ordnance Laboratory
Winkler, E. M., Dr.	U. S. Naval Ordnance Laboratory
Wise, W. R.	U. S. Naval Ordnance Laboratory
Woodward, E. H.	Naval Research Laboratory
Woolston, L. L.	U. S. Naval Ordnance Laboratory
Young, Martin	International Business Machines
Zakhartchenko, C.L., Dr.	
Zawatsky, A.	U. S. Naval Ordnance Laboratory
Zebb, Keirn	So. Calif. Co-op Wind Tunnel
Zima, Wm.	Wright Patterson Air Force Base
Zirnkilton, F. C.	Allis Chalmers Manufacturing Company

APPENDIX B

Detail Program for the Aeroballistic
Dedication - Decennial Affair
25 - 26 May 1959

Dedication Program
Monday, 25 May 1959

0830 Registration, Auditorium, NOL

Time	
0920	Call to Order by Chairman Dr. H. H. Kurzweg, Associate Technical Director for Aeroballistic Research, NOL
0925	Welcome Captain Mell A. Peterson, Commander, NOL
0935	Presentation of New Aeroballistic Research Facilities Vice Admiral John T. Hayward, Deputy Chief of Naval Operations for Development
0950	Address Rear Admiral W. F. Raborn, Director Special Projects Office, BuOrd
1005	Response Dr. G. K. Hartmann, Technical Director, NOL
1020	Recess
1040	The Aeroballistic Research Facilities at NOL Dr. H. H. Kurzweg
1120	Contributions of Aeroballistics to Space Explorations Dr. H. L. Dryden, Deputy Administrator, NASA
1155	Announcements by Chairman
1205	The Decennial Program Dr. R. E. Wilson, Aeroballistics Program Chief, NOL

Time	
1220	Tour Instructions Mr. J. R. Lightfoot, Assistant Aeroballistics Program Chief for Engineering, NOL
1230	Lunch
1400	Tour of Aeroballistic Research Facilities
1700	Social Hour
1800	Dinner
1930	After-dinner Speech Dr. Theodore von Karman, Chairman Advisory Group for Aeronautical Research and Development, NATO

\- - - - - -

Decennial Program
Tuesday, 26 May 1959

0845	Opening Remarks by Chairman Dr. R. J. Seeger, Deputy Assistant Director Mechanical, Physical and Engineering Sciences, NSF
0900	High-Temperature Gas Dynamics Dr. J. M. Burgers, Institute for Fluid Dynamics and Applied Mathematics, University of Maryland
0935	Survey of NOL Aerodynamics Research Dr. R. K. Lobb, Chief Aerodynamics Department, NOL
1005	Survey of NOL Hyperballistics Research Dr. Z. I. Slawsky, Chief Ballistics Department, NOL
1035	Recess
1100	Some Recent Advances in the Mechanics of Highly Rarefied Gases Dr. G. N. Patterson, Director Institute of Aerophysics, University of Toronto

Time	
1135	Survey of NOL Applied Mathematics Research Dr. R. C. Roberts, Chief Mathematics Department, NOL
1205	Effects of Mass Transfer on Boundary-Layer Characteristics Mr. I. Korobkin, Chief Aerophysics Division, NOL
1235	Summary Remarks by Chairman
1245	Lunch
1355	Opening Remarks by Chairman Dr. R. E. Wilson, Aeroballistics Program Chief, NOL
1400	Address Dr. Wernher von Braun, Director Development Operations Division Army Ballistic Missile Agency
1445	Some Comments on the Phenomena of High-Speed Impact Dr. A. C. Charters, Chief Hypervelocity Ballistic Range Branch Ames Research Center, NASA
1525	Millisecond Measurements of Forces and Moments in Hypersonic Flow Dr. A. E. Seigel, Chief Gas Dynamics Division, NOL
1555	Recess
1615	Mathematical Aspect of Supersonic Nozzle Design Dr. W. R. Thickstun, Chief Mathematical Analysis Division, NOL
1645	Summary Remarks by Chairman
1650	Adjournment

APPENDIX C

TOUR PLAN

25 May 1959

Formal Tour: 1400 - 1545 Informal Inspection: 1545 - 1700

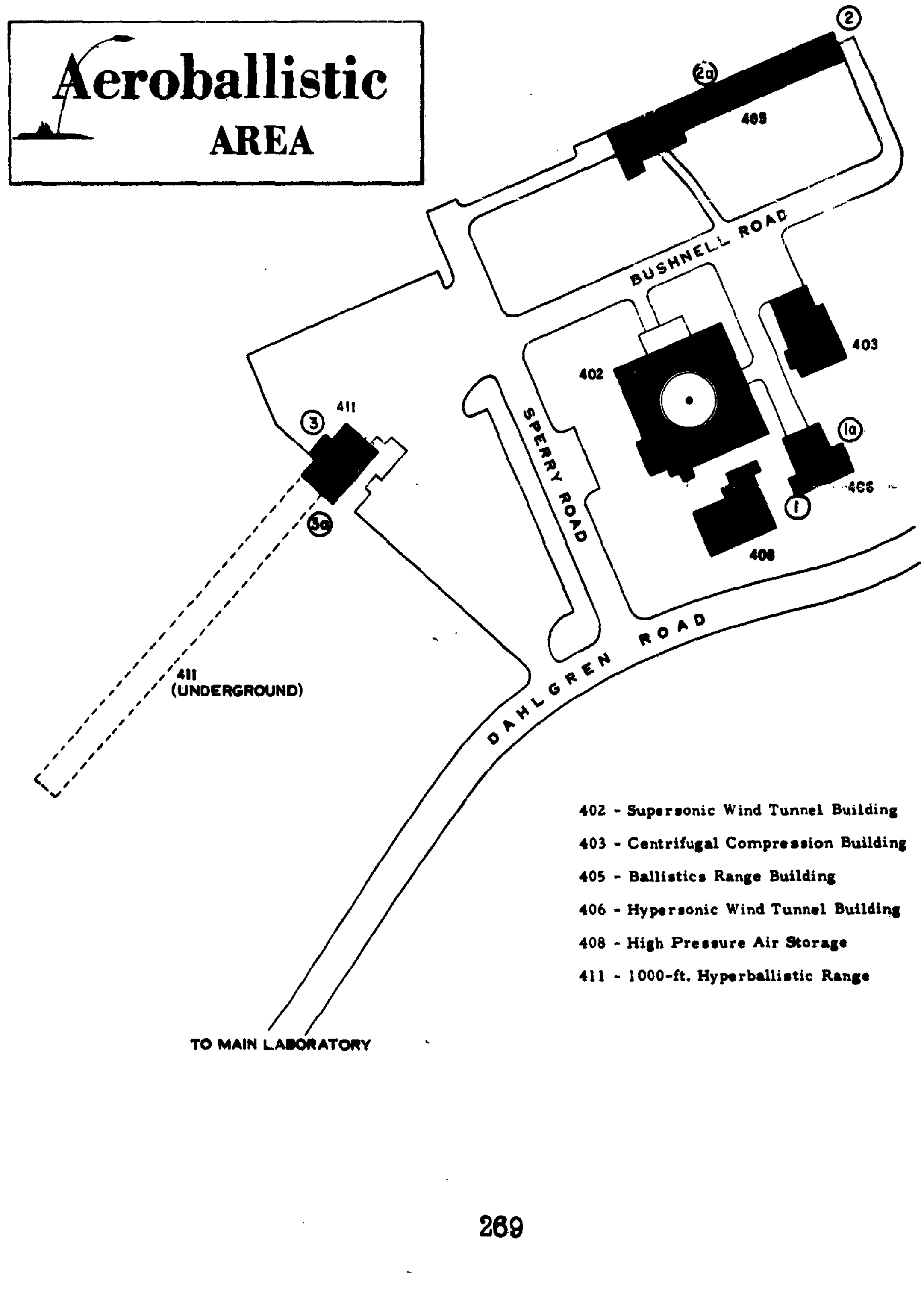
Stop No.	Location	Demonstration
1	Building 406 (West side)	Hypersonic Wind Tunnel No. 8 Lectures by: Dr. Eva Winkler Mr. Irving Shantz Mr. Robert Schroth
1-a	Building 406 (East side)	Inspection of New Hypersonic Wind Tunnel No. 8
2	Building 405 (East end)	4-in. Hypersonic Shock Tunnel No. 3 Lecture by: Mr. V. C. Dawson
2-a	Building 405 (Center)	Inspection of New 4-in. Hypersonic Shock Tunnel No. 3
3	Building 411 (East end)	1,000-ft. Hyperballistics Range No.4 Lecture by: Mr. P. A. Thurston
3-a	Building 411	Inspection of New 1,000-ft. Hyperballistics Range No. 4 and 4-in. Gas Gun

----

Informal Inspection of all:

Supersonic Wind Tunnels
Hypersonic Wind Tunnels
Ballistic Ranges
Hypersonic Shock Tunnels

APPENDIX C



Aeroballistic
AREA
2
2a
405
BUSHNELL ROAD
403
402
411
3
1a
406
1
3a
SPERRY ROAD
408
ROAD
DAHLGREN
411
(UNDERGROUND)
402 - Supersonic Wind Tunnel Building
403 - Centrifugal Compression Building
405 - Ballistics Range Building
406 - Hypersonic Wind Tunnel Building
408 - High Pressure Air Storage
411 - 1000-ft. Hyperballistic Range
TO MAIN LABORATORY

APPENDIX D

THE U.S. NAVAL ORDNANCE LABORATORY
WHITE OAK, MARYLAND
invites you to the formal

Dedication

of the new

Aeroballistic Research Facilities

and the

Decennial Symposium

INVITATION

25 AND 26 MAY 1959

Request for Registration

I PLAN TO ATTEND THE FOLLOWING:

Check

- ☐ Dedication Ceremony and Tour, 25 May 1959
- ☐ Decennial Symposium, 26 May 1959
- ☐ Social Hour and Dinner at NOL, 25 May 1959, P.M.
- ☐ Use Navy Dept. Bus from Washington, D.C. (Roger Smith Hotel) and return, 25 and 26 May 1959

I am / am not a citizen of the U.S.A.

SIGNED ______________________

COMPANY ADDRESS ______________________